

THURSDAY, NOVEMBER 29, 1877

## FLORA OF MAURITIUS AND SEYCHELLES

*Flora of Mauritius and the Seychelles: a Description of the Flowering Plants and Ferns of those Islands.* By J. G. Baker, F.L.S. (London: L. Reeve and Co., 1877.)

THIS compact volume of nearly 600 pages, adds another to the already long list of colonial floras prepared at Kew and issued under the authority and at the expense of the Colonial Government. It is arranged on the same plan as the other floras, many of them so well known, giving first, some general remarks on the physical geography and botany of the islands, and then that admirable outline of elementary botany prepared by Mr. Bentham, and which contains every definition necessary in descriptive botany, thus enabling the student to follow the technical descriptions given in the "Flora" itself. The work is almost entirely from the pen of Mr. J. G. Baker (the Orchids being by Mr. Le Marchant Moore, and the Palms and Pandani by Dr. I. B. Balfour), and is only another example of the indomitable industry so characteristic of Mr. Baker. The materials at the disposal of the author have been ample, and probably there is but little left to discover in Mauritius, the Seychelles, and Rodriguez, although many forms have not as yet been fully determined owing to the want of perfect specimens. Hence it is desirable that naturalists visiting the islands should endeavour to complete our knowledge of these imperfectly known plants. The smaller dependencies of Mauritius have not been explored botanically, hence there is probably a rich field for the investigator of these numerous islands. It is, moreover, all the more desirable to have these islands explored as the native flora of the islands already known has been completely altered by the introduction of cultivated plants and weeds as well as by the destruction of the native forests. Thus it is probable that in some of the undisturbed islands a rich native flora may be met with, or that some of the forms either rare or extinct on other islands, may yet be comparatively abundant.

Mauritius is about 39 miles by 35, and has an area of 700 square miles, or a little smaller than the County of Surrey. It is situated at a distance of about 500 miles from Madagascar and 100 miles from Bourbon, and is just within the Tropic of Capricorn. The northern part of the island is a low plain covered with sugar plantations. In the centre is an elevated plateau rising to about 1,500 feet above the sea-level, the great mass of the rocks being entirely volcanic. Outside the central plateau, and within a short distance of the sea, rise the three principal mountain ranges, the highest portions being from 1,900 to 2,900 feet in height. There are two small lakes in the central plateau, the Grand Bassin and the Mare aux Vacoas. There are six rivers, about ten to twelve miles in length, and numerous small rivulets. The climate is warm, and at Port Louis the mean annual temperature is 78° F. As a result, the vegetation has a decidedly tropical character. There are however, a few south temperate plants present, and also a number of the widely-spread temperate forms, as *Nephrodium filix-mas*, *Cardamine hirsuta*, *Juncus effusus*, *Convolvulus arvensis*, *Plantago major*, and *P. lanceolata*.

Sugar is extensively cultivated in Mauritius. The increase in the cultivation of sugar has led to the destruction of the forests, which at one time covered the island to the water's edge. As a result of the destruction of the forests, the indigenous flora has almost become destroyed. The orchids, ferns, pandani, and the shade-loving plants, and the curious endemic trees and shrubs have, within 100 years, been either entirely exterminated, or else have become exceedingly rare and local. The native vegetation thus partly exterminated has been replaced by a number of introduced trees, shrubs, and weeds, to an extent only exceeded by the destruction of the indigenous flora of St. Helena. There seem to be about 269 introduced plants in Mauritius, and 869 undoubtedly native species, making a total flora of about 1,138.

The Seychelles are situated 900 miles north-east of Mauritius, in 3°-6° south latitude, and consist of a group of about thirty islands, most of them of very small size. The islands are entirely granitic. The largest of the group, Mahé, has an area of 30,000 acres; the best cultivated and most populous is La Digue, with an area of 2,000 acres. The mountains range from about 1,500 to 3,000 feet in height. The seasons are similar to those of Mauritius. Cotton was at one time extensively cultivated, and the aboriginal forests were destroyed to make room for cotton plantations. Now cotton is hardly cultivated, the chief exports from the island being cocoa-nut oil and fibre. The vegetation is wholly tropical; the few temperate species found in Mauritius being absent from the Seychelles. The number of flowering plants and ferns from these islands is 338. Five genera of palms and one genus of *Ternstroemiaceæ* are endemic. The endemic palms are mostly well known, and belong to the genera *Deckenia*, *Nephrosperma*, *Roscheria*, *Verschaffeltia*, *Lodoicea*, and *Stevensonia*. The total number of endemic species is sixty. The rest of the flora consists chiefly (250) of widely distributed tropical plants, and between twenty and thirty are of characteristic Mascarene types. The flora was expected to have been much richer in endemic forms from the isolated position and peculiar geological construction of the islands than it has proved to be after the most careful examination.

Rodriguez is situated 300 miles to the north and east of Mauritius, and is an island about eleven miles by five, with the hills in the interior reaching an elevation of little over 1,000 feet. The rock is entirely volcanic, and the climate similar to that of Mauritius. The flora must have undergone great changes, as the earliest records of the island state that it was entirely wooded. The plants of the island number about 202 wild flowering plants and ferns, nearly all collected by that rising young botanist, Dr. I. B. Balfour, one of the staff of the Transit of Venus Expedition to Rodriguez. Of the 202 wild species, thirty-six are peculiar to the island; and there are three endemic monotypic genera, one *Mathurina* having been discovered and described by Dr. I. B. Balfour.

The total number of species as given by Baker may be thus summarised:—There are 1,058 native species in the "Flora," 869 natives of Mauritius, 338 natives of Seychelles, and 202 native in Rodriguez; 269 are naturalised in these islands, thus giving a total number of 1,327 species included in the "Flora of Mauritius and the Seychelles." The distribution of the species in the flora

is also interesting. Thus there are 304 endemic species, 232 Mascarene species, i.e., plants confined to Bourbon, Mauritius, Madagascar, and the Comoros; 66 African but not Asian, 86 Asian but not African; 145 common to Asia and Africa; and 225 common to the Old and New World. If we take the percentages we have the following results:—29 per cent. endemic, 22 per cent. Mascarene, 21 per cent. common to the Old and New World, 14 per cent. common to Asia and Africa, 8 per cent. Asian but not African, and 6 per cent. African but not Asian. From this it is evident that one-half of the wild plants of the flora are restricted to the Mascarene Archipelago.

The orders containing the greatest number of species are the following:—Orchidaceæ, 79; Gramineæ, 69; Cyperaceæ, 62; Rubiaceæ, 57; Euphorbiaceæ, 45; Compositeæ, 43; Leguminosæ, 41; Myrtaceæ, 20. There also 168 species of Filices, but it is rather unfair to consider the Filices as an order equivalent say to the Euphorbiaceæ or Myrtaceæ in the above enumeration.

The descriptive part of the flora is elaborated in the same manner as the colonial floras already published, and is, as already mentioned, almost entirely the work of Mr. Baker, with the exception of the Orchids, Palms, and Pandani. Any one acquainted with Mr. Baker's work will know that any detailed notice of the descriptive part of the present volume is superfluous.

W. R. McNAB

#### OUR BOOK SHELF

*Die Geologie.* Franz Ritter von Hauer. (Vienna : A Holder, 1877.)

IT is a good sign both of the progress of geological study in Austria and of the value of this manual by the director of the Austrian Geological Survey, that a second edition of the work has been called for within three years of the date of its publication. A sample of the revised issue which has been sent to us fully bears out the description on its title-page that it is enlarged and improved. The original work, besides its clearly-expressed introductory chapters on general dynamical and mineralogical geology, is especially a valuable repertory of information regarding the structure and paleontology of the Austro-Hungarian monarchy. In the new edition, Ritter von Hauer is evidently doing his best to keep his manual abreast of the time. The book is well-printed, but the author is still in the hands of a very poor wood-engraver. The new cuts are as rude and feeble as ever.

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

#### Fritz Müller on Flowers and Insects

THE enclosed letter from that excellent observer, Fritz Müller, contains some miscellaneous observations on certain plants and insects of South Brazil, which are so new and curious that they will probably interest your naturalist readers. With respect to his case of bees getting their abdomens dusted with pollen while gnawing the glands on the calyx of one of the Malpighiacæ, and thus effecting the cross-fertilisation of the flowers, I will remark that this case is closely analogous to that of Coronilla

recorded by Mr. Farrer in your journal some years ago, in which parts of the flowers have been greatly modified, so that bees may act as fertilisers while sucking the secretion on the outside of the calyx. The case is interesting in another way. My son Francis has shown that the food-bodies of the Bull's-horn Acacia, which are consumed by the ants that protect the tree from its enemies (as described by Mr. Belt), consist of modified glands; and he suggests that aboriginally the ants licked secretion from the glands, but that at a subsequent period the glands were rendered more nutritious and attractive by the retention of the secretion and other changes, and that they were then devoured by the ants. But my son could advance no case of glands being thus gnawed or devoured by insects, and here we have an example.

With respect to *Solanum palinacanthum*, which bears two kinds of flowers on the same plant, one with a long style and large stigma, the other with a short style and small stigma, I think more evidence is requisite before this species can be considered as truly heterostyled, for I find that the pollen-grains from the two forms do not differ in diameter. Theoretically it would be a great anomaly if flowers on the same plant were functionally heterostyled, for this structure is evidently adapted to insure the cross-fertilisation of distinct plants. Is it not more probable that the case is merely one of the same plant bearing male flowers through partial abortion, together with the original hermaphrodite flowers? Fritz Müller justly expresses surprise at Mr. Leggett's suspicion that the difference in length of the pistil in the flowers of *Pontederia cordata* of the United States is due to difference of age; but since the publication of my book Mr. Leggett has fully admitted, in the *Bulletin* of the Torrey Botanical Club, that this species is truly heterostyled and trimorphic. The last point on which I wish to remark is the difference between the males and females of certain butterflies in the neuration of the wings, and in the presence of tufts of peculiarly-formed scales. An American naturalist has recently advanced this case as one that cannot possibly be accounted for by sexual selection. Consequently, Fritz Müller's observations which have been published in full in a recent number of *Kosmos*, are to me highly interesting, and in themselves highly remarkable.

CHARLES DARWIN

Down, Beckenham, Kent, November 21

YOU mention ("Different Forms of Flowers," page 331), the deficiency of glands on the calyx of the cleistogamic flowers of several Malpighiacæ, suggesting, in accordance with Kerner's views, that this deficiency may be accounted for by the cleistogamic flowers not requiring any protection from crawling insects. Now I have some doubt whether the glands of the calyx of the Malpighiacæ serve at all as a protection. At least, in the one species, the fertilisation of which I have very often witnessed, they do not. This species, *Bunchosia gaudichaudiana*, is regularly visited by several bees belonging to the genera Tetrapedia and Epicharis. These bees sit down on the flowers gnawing the glands on the outside of the calyx, and in doing so the under side of their body is dusted with pollen, by which, afterwards, other flowers are fertilised.

There are here some species of *Solanum* (for instance *S. palinacanthum*) bearing on the same plant long-styled and short-styled flowers. The short-styled have papillæ on the stigma and apparently normal ovules in the ovary, but notwithstanding they are male in function, for they are exclusively visited by pollen-gathering bees (*Melipona*, *Euglossa*, *Augochlora*, *Megacilissa*, *Eophila*, n. g., and others), and these would probably never insert their proboscis between the stamens.

In a few months I hope to be able to send you seeds of our white-flowered violet with subterranean cleistogamic flowers. I was surprised at finding that on the Serra (about 1,100 metres above the sea) this violet produced abundant normal fruits as well as subterranean ones, while at the foot of the Serra, though

it had flowered profusely, I could not find a single normal fruit, and subterranean ones were extremely scarce.

According to Delpino the changing colours of certain flowers would serve to show to the visiting insects the proper moment for effecting the fertilisation of these flowers. We have here a Lantana the flowers of which last three days, being yellow on the first, orange on the second, purple on the third day. This plant is visited by various butterflies. As far as I have seen the purple flowers are never touched. Some species inserted their proboscis both into yellow and into orange flowers (*Danais erippus*, *Pieris aripa*), others, as far as I have hitherto observed, exclusively into the yellow flowers of the first day (*Heliconius apaeudes*, *Colanis julia*, *Eurema lutea*). This is, I think, a rather interesting case. If the flowers fell off at the end of the first day the inflorescence would be much less conspicuous; if they did not change their colour much time would be lost by the butterflies inserting their proboscis in already fertilised flowers.

In another Lantana the flowers have the colour of lilac, the entrance of the tube is yellow surrounded by a white circle; these yellow and white markings disappear on the second day.

Mr. Leggett's statements about *Pontederia cordata* appear to me rather strange, and I fear that there is some mistake. In all the five species of the family which I know the flowers are so short-lived, lasting only one day, that a change in the length of the style is not very probable. In the long-styled form of our high- and Pontederia the style has its full length long before the flowers open. In my garden this Pontederia is visited by some species of Augochlora collecting the pollen of the longest and mid-length stamens; they are too large to enter the tube of the corolla, and have too short a proboscis to reach the honey; they can only fertilise the long-styled and mid-styled forms, but not the short-styled.

Among the secondary sexual characters of insects the meaning of which is not understood, you mention ("Descent of Man," vol. i., p. 345) the different neuration in the wings of the two sexes of some butterflies. In all the cases which I know this difference in neuration is connected with, and probably caused by, the development in the males of spots of peculiarly-formed scales, pencils, or other contrivances which exhale odours, agreeable no doubt to their females. This is the case in the genera Mechanitis, Diricenna, in some species of Thecla, &c.

FRITZ MÜLLER

Blumenau, St. Catharina, Brazil, October 19

#### The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS'S letter in NATURE (vol. xvii., p. 26) has directed attention prominently to the circumstance that two hypotheses have been submitted to the scientific world as explanations of the force and motions which Mr. Crookes had shown to exist—one by Prof. Osborne Reynolds, the other by myself.

Prof. Osborne Reynolds's explanation is based on the fact that when a disc with vertical sides is heated on one side and exposed to a gas, a convection current sets in, which draws a continuous supply of cold gas into contact with the hot surface of the disc. As this cold gas reaches the disc it is expanded, and thus its centre of gravity is thrown further from the disc. Accordingly, the disc, if freely suspended, will move in the opposite direction so as to keep the centre of gravity of the gas and disc in the same vertical line as before, and, if not freely suspended, will suffer a pressure tending to make it move in that direction. If I have understood Prof. Reynolds aright, this is both a correct and full description of his explanation as last presented.

My explanation, on the other hand, is based on molecular motions which go on in the gas without causing any molar motion, and is independent of convection currents. Prof. Reynolds is therefore, I conceive, fully justified in denying that my theory has supplied any deficiency in his explanation. As he points out, the two explanations are incompatible; if either is correct, the other is wholly wrong.

It is easy to apply comparative tests to the rival hypotheses by

making a selection from Mr. Crookes's incomparable experiments, from the experiments by Mr. Moss and myself, and from instances of compressed Crookes's layers in the open atmosphere; but it is not easy to make the choice so as to bring the abundant evidence within the compass of a letter.

These tests might take various forms, of which perhaps the most direct is to ascertain whether the force is affected by variations in the convection current, as required by Prof. Reynolds's hypothesis, or is independent of convection, but increased when the heater and cooler are brought nearer together, as required by mine.

To test this Mr. Crookes mounted a radiometer in a receiver consisting of two unequal bulbs connected by a large tube. The movable portion could be transferred from one bulb to the other through the tube. In the small bulb the convection current is most impeded, and at the same time the heater and cooler are closest together. Mr. Crookes found that the motion of the radiometer was more rapid in the small bulb than in the large one, in conformity with my theory, and in opposition to Prof. Reynolds's. The same is the uniform drift of a vast number of other experiments by Mr. Crookes, and of those by Mr. Moss and myself, from which it appears that whenever the heater and cooler are made to approach there is an increase in the force, and that the force is not appreciably affected by variations of the convection current, or by its suppression.

This may also be proved, and quite conclusively, by observations not requiring apparatus. Drops in the spheroidal state and the drops which are often seen floating on the surface of volatile liquids, as, for example, the drops which run about on the surface of the sea in certain states of the weather when water drips from an oar, are supported by Crookes's layers of air intervening between them and the liquid beneath. Similarly a red-hot copper plate will float on water, supported on a Crookes's layer, and many other instances of a like kind might be adduced. In such cases, where the film of air is thin and for the most part horizontal, it is manifest that there is no opportunity for those convection currents to arise which are required by Prof. Reynolds's hypothesis, while in all of them there are the peculiar molecular motions of my theory.

The absence of convection currents which could produce an appreciable effect may also be proved in those radiometers of which the arms whisk round at a very rapid speed, and in many other cases that would take too much space to describe here.

Again, a tangential force which may be rendered considerable is an immediate consequence of my theory, but has no place as a consequence of Prof. Reynolds's. Now its presence has been verified by Mr. Moss and myself, and by Mr. Crookes in an exquisitely beautiful apparatus suggested for this purpose by Prof. Stokes, as well as, in a less degree, in all Mr. Crookes's apparatus with curved or crumpled discs.

Hence Prof. Osborne Reynolds's hypothesis is not the explanation of Crookes's stress. It alleges a cause which is in certain cases a *vera causa*, but not the cause of what is to be explained. So far as I can form a judgment, its merit was collateral, and not intrinsic. It was the first attempt at a reduction of the observed phenomena to known physical laws. Though not accounting for them, it was sufficiently plausible to attract the attention of Prof. Reynolds and other physicists. It thereby had the important effect of suggesting Dr. Schuster's most valuable experiment, which was the first that established the cardinal fact that the forces within a radiometer case are balanced.

The conclusion to which we are thus led by a purely experimental inquiry is supported by an examination of the chief theoretic assertions of Prof. Osborne Reynolds's letter, viz., 1. That an essential part of my explanation "is contrary to the law of the diffusion of heat in gases"; and 2. "That the force arising from the communication of heat from a surface to adjacent gas of any particular kind depends only on one thing, the rate at which heat is communicated, and to this it is proportional."

Both of these statements have been set down by Prof. Osborne Reynolds in error; the first from not observing that the ordinary laws for the propagation of heat through a gas do not apply to compressed Crookes's layers; and the second from a misapprehension of the actual agency at work in radiometers and other similar apparatus. I will proceed to establish these two positions.

1. So long as a gas is in its ordinary state the distribution of the velocities of the molecules is the same in all directions, and when heat is imparted to the gas it does not disturb this uniformity of structure. The heat simply increases the mean velocity, and the actual velocities continue to be distributed about

their mean value according to the well-known exponential law, and are alike in all directions. But the gas of a compressed Crookes's layer is not in the ordinary state; it is under constraint, as I have elsewhere shown, owing to the proximity of the heater and cooler between which it is confined. In consequence of this constraint there are what I have described as processions going on in the layer of gas: in other words, *the velocities of the molecules at any situation within the layer are not alike in all directions, but are greatest in the direction of the cooler, least in the direction of the heater, and of intermediate values in lateral directions.* The heat in crossing the layer from the heater to the cooler maintains this polarised molecular structure, and if the flow of heat is increased it does not simply increase the mean velocity of the molecules, but also augments the disparity of the velocities in different directions.

Now the ordinary laws of the communication of heat to and through gas are based on the opposite supposition that when heat reaches any portion of the gas all the molecules of that portion are equally affected, that though their mean velocity is increased the laws of the distribution of the velocities about that mean, and in different directions, are not changed. Hence Prof. Osborne Reynolds has fallen into an error in applying the ordinary "law of the diffusion of heat in gases" to the case of compressed Crookes's layers. The law employed by Prof. Reynolds does not prevail unless there is sufficient room in front of the heater for the development of a complete *unrestricted* Crookes's layer; Crookes's force only presents itself when the thickness of that layer is restricted by a cooler.

The transmission of heat across Crookes's layers is made the subject of investigation in a memoir which I laid before the Royal Dublin Society last May, which has recently been printed in the *Transactions* of that body, and of which a copy will shortly appear in the *Philosophical Magazine*. The law proves to be entirely different from any of the laws for the propagation of heat hitherto known, and I have therefore called this mode of transferring heat by a new name—*the penetration of heat*. Moreover, the results of theory had been verified by anticipation more than thirty years before by MM. De la Provostaye and Desains, in two elaborate experimental investigations into what we now know to have been the penetration of heat; so that our knowledge of its laws, which are entirely different from the laws of the diffusion of heat, quoted by Prof. Reynolds, already stands on both a deductive and experimental basis.

2. Prof. Osborne Reynolds further states that with each gas the force depends only on one variable, viz., the rate at which heat is communicated by the heater to the adjacent gas, and that it is proportional to this rate. Probably owing to a mere slip on Prof. Reynolds's part, he has here omitted a second variable, viz., the temperature of the gas, which is implicitly contained in the equation of his first paper to which he refers. With this, however, I have no concern; what I have to point out is that in making the statement, whether in an amended or in its actual form, Prof. Osborne Reynolds has overlooked the fact that the machinery of Crookes's stress consists of a cooler as well as of the heater and intermediate gas, and that *a sufficient proximity of the cooler is essential*. Accordingly, the true expression for the force (of which I hope to publish an investigation made some time ago, as soon as my health will allow) is not so simple as Prof. Reynolds supposes, but is a function of the temperatures of the heater and cooler, and of the rate at which heat reaches the cooler by penetration, in addition to the single variable which one Prof. Osborne Reynolds admits. The vice of the mathematical reasoning, on which Prof. Reynolds bases his statement, is that it starts from a kinetic expression for the pressure of gas, which is only true when the mean of the squares of the velocities of the molecules is the same in all directions. Accordingly, his discussion does not reach the phenomenon it professes to explain; it is irrelevant to the case of compressed Crookes's layers, in which the gas is polarised, and where the degree of polarisation is itself a function of Prof. Reynolds's variable along with other thermal variables.

Thus, in all parts of his inquiry, Prof. Osborne Reynolds has been led into error by having regarded the gas of compressed Crookes's layers as gas in its ordinary state; in other words, because he has not had a glimpse of that peculiar molecular structure in the gas, which is the real source of Crookes's stress. From a review of the whole subject I think myself justified in submitting that the only discovery which brought with it any knowledge of the cause of Crookes's stress and of penetration, was the discovery that gas could assume this polarised condition; and I must say that it does not appear to me that

to this discovery Prof. Osborne Reynolds has in any degree contributed.

Dublin, November 15

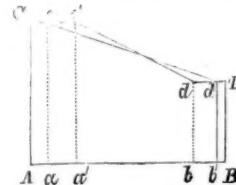
G. JOHNSTONE STONEY

Postscript, November 23.—Prof. Osborne Reynolds has written a further letter to NATURE (vol. xvii. p. 61), in which he says:—"The fact that Mr. Stoney has in no way referred to my work, although I preceded him by some two years, has relieved me from all obligation to discuss Mr. Stoney's theory." I am sorry Prof. Osborne Reynolds should have thought me capable of courtesy or inattention to the claims of a fellow-worker, and fortunately I am not conscious of being liable to the imputation. I became acquainted with Prof. Reynolds's paper in the interval between the publication of my first and second papers, but did not refer to it in my second paper because I found on reading it that Prof. Reynolds's explanations of Crookes's force were all erroneous (viz., the evaporation of mercury or other vapour, and heat communicated to diffused particles of gas, or to gas brought by convection currents); because the mathematical analysis with which he supports his hypotheses is irrelevant to the problem with which he is dealing; and finally, because for the purposes of my investigation I had no occasion to point out these mistakes, inasmuch as Prof. Reynolds had not approached the subject of polarised layers of gas and their mechanical properties, which was the subject matter of my papers.

I ought to add a word in reference to the criticism of my memoir on penetration, which is contained in Prof. Osborne Reynolds's last letter. He seems to overlook a condition laid down in the second paragraph of my memoir, which disposes of the criticism, viz.: "Let us further regard this gas as a *perfect non-conductor of heat*." Your mathematical readers will at once perceive that this condition is a legitimate simplification of the problem, because the diffusion or conduction of heat in gases is very sluggish compared with penetration, the phenomenon with which I was dealing.

It appears from Prof. Osborne Reynolds's last letter that my wish to make my note to NATURE (vol. xvii. p. 43) a fortnight ago short, led me to make it obscure. I will therefore, with your permission, try to state the matter more clearly.

As I understand the scientific question in discussion before us, it is this:—Assuming (1) that, when heat is communicated from a solid surface to a gas in contact with it, a force arises (equivalent to a pressure against the surface) which is proportional to the rate of communication of heat, and (2) that the conducting power of a gas for heat is independent of its density, Prof. Reynolds concludes that the driving-force on the vanes of a radiometer does not increase with the rarefaction of the air, but that rarefaction favours the motion only in so far as it lessens the opposing force due to convection-currents. I, on the other hand, while admitting Prof. Reynolds's premisses, do not admit his conclusion. On the contrary, I believe that, in the radiometer, rarefaction must increase the rate of communication of heat, and hence also the force. To see how this may be, let  $A$   $B$  represent the thickness of a stratum of gas contained between two parallel solid surfaces, whose temperatures, measured from any zero, are represented respectively by  $A$   $C$  and  $B$   $D$ . Then, I imagine, the flow of heat through the gas will take place as though there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as that of the contiguous solid, and whose thickness is equal (or at least proportional) to the mean length of path of the molecules. The virtual thickness of the stratum of gas, whose conductivity comes into account in determining the rate of transmission of heat, is then the actual thickness diminished by the aggregate thicknesses of these two layers. For example, if  $A$   $a$  and  $b$   $B$  represent the thicknesses of the hot and cold layers respectively, the virtual



thickness of the stratum across which conduction takes place is  $a$   $b$ , and the distribution of temperature from side to side of the whole quantity of gas is given by the ordinates of the

broken line  $C'D'$ . If now the gas is rarefied, the mean length of path of the molecules, and consequently the thickness of each of the layers of uniform temperature, is increased, and the thickness of the stratum across which true conduction takes place is diminished. If, for example, the thicknesses of the layers become  $A'a'$  and  $B'b'$ , the thickness of the conducting stratum is reduced to  $a'b'$ , and the distribution of temperature is represented by the ordinates of the broken line  $C'D'$ . The rate of flow of heat in the two cases will be proportional conjointly to the inclination of the line  $cd$  or  $c'd$  to  $AB$ , and to the conductivity of the gas; but as the latter factor does not vary with density, the result is proportional to the former only. It is evident that if this view of the matter is approximately correct rarefaction must increase the rate of transmission of heat across a stratum of gas whenever the increased length of path of the molecules, resulting from rarefaction, bears an appreciable proportion to the thickness of the stratum, but that it will have no sensible effect of the kind when the stratum of gas is very thick or the rarefaction itself very small.

I ought to acknowledge that precisely this mode of representing the effect of rarefaction occurred to me only as I was thinking how I could comply with Prof. Osborne Reynolds's wish that I should be "more explicit." When I wrote my last note I had in mind a somewhat different mode of action whereby it seemed that an equivalent result to that here pointed out would be brought about. The further consideration which Prof. Reynolds's letter in this week's NATURE has caused me to give to the subject has, however, led me to think that the view given above is not only clearer, but also a nearer approach to a correct representation of the facts than the one I had previously adopted. But apart from the accuracy of any particular explanation of how such a result can occur, the experimental evidence seems to me to prove conclusively that the force in the radiometer does increase (up to a certain point) with rarefaction. The action of convection currents depends to so great an extent on such conditions as the size and shape of the envelope and the position of the fly, and they must be so much disturbed as soon as the vanes begin to move, that if they played the essential part which I understood Prof. Reynolds to attribute to them, I cannot think that the effect of rarefaction would present anything like the degree of regularity that has been actually observed.

November 24

G. CAREY FOSTER

#### Mr. Crookes and Eva Fay

THE precise nature and grounds of the attestation given by Mr. Crookes to Eva Fay's "mediumship" appear in an article entitled "Science and Spiritualism" in the *Daily Telegraph* for March 13, 1875, embodying a communication made by Mr. Crookes himself to the *Spiritualist* of the preceding day.

The readers of NATURE will be able to judge for themselves by the following extracts from this article, whether Eva Fay was not fully justified in announcing her "mediumship" to the American public as having received Mr. Crookes's endorsement."

"In the *Spiritualist* of yesterday, Mr. William Crookes, F.R.S., prints an account of a séance at his house in which Mrs. Fay exhibited some remarkable phenomena while under severe scientific conditions. The sitting took place on Friday evening, February 19, in the presence of several well-known men of science; and, on Mr. Crookes's suggestion, the medium was so placed as to form part of an electrical current connected with a galvanometer, indicating on a graduated circle the exact deflection produced by the current. In each hand Mrs. Fay held the terminal of a wire, and the fact that she kept continuous hold of the terminals was guaranteed by the amount of deflection of the galvanometer needle, and by the flashes of light which accompany each change of position or break of contact. This method was agreed to by the *savants* present, as affording absolute certainty that the medium could not remove her hand or body from the wires, whether in a trance or otherwise, without the fact being made known by the galvanometer. The sitting was held in a well-lighted drawing-room, the medium thus 'tied down by electricity' being screened by a curtain. What followed is thus described by Mr. Crookes:

"We commenced the tests at 8.55 P.M.; the deflection by the galvanometer was 211 deg., and the resistance of Mrs. Fay's body 6,600 British Association units. At 8.56 the deflection was 214 deg., and at this moment a handbell began to ring in the library. At 8.57 the deflection was 215 deg. A hand came out of the cabinet on the side of the door farthest from Mrs. Fay."

A number of other occurrences of the like kind are then recorded; the hand reappearing from time to time, and presenting

different members of the party with books and other articles severally appropriate to each, of which Mr. Crookes considered it impossible that Mrs. Fay could herself have gained possession.

He adds:—"Before Mrs. Fay came to the house that evening, she only knew the names of two of the guests who would be present; but during the evening the intelligence at work displayed an unusual amount of knowledge about the sitters and the labours of their lives."

The entire extract (which I should have reproduced in full if the space of NATURE had permitted) would show that—I. It is true that Mr. Crookes gave his *public attestation* to the genuineness of the so-called spiritualistic manifestations which occurred in his house through the "mediumship" of Eva Fay.

2. It is true that Eva Fay went back to the United States armed with Mr. Crookes's *public attestation* of the genuineness of the performances which took place at his house.

3. It is true that Mr. Crookes wrote a letter to a gentleman in the United States, giving a similar attestation, which letter was published in *facsimile* in an American newspaper.—The only thing that was *not* true in my statement, was that (through having mislaid the slip containing it) I spoke of this letter as having been addressed to Eva Fay herself, and having been written before her departure.

4. It is true that Eva Fay's *public performances* in London were imitated at the time by Messrs. Maskelyne and Cooke; and further, that her business agent *spontaneously offered* Mr. Maskelyne to expose (for a sum of money) the tricks by which she cheated "the F.R.S. people."—If NATURE thinks it worth while to admit into its columns the full particulars of that offer, Mr. Maskelyne is quite ready to furnish them. His general assertion of the fact has been long before the public ("Modern Spiritualism," p. 122), and has remained unchallenged, so far as I am aware, until now.

5. It is true that the whole *modus operandi* of Eva Fay's *public manifestations* in the United States has been publicly exposed in New York and Boston by Mr. Washington Irving Bishop, as stated in *Fraser's Magazine* for the present month.

It was not only in entire ignorance of these proceedings, but under the influence of a report in circulation among the Fellows of the Royal Society—that "Mr. Crookes had given up Spiritualism," that I expressed to Mr. Crookes, on the occasion of his receiving the Royal Medal, my desire to "bury the hatchet." But I most assuredly did not consider myself thereby pledged to keep silence in regard to any further proceedings of the like kind; and only learned at the beginning of the present year that Eva Fay had been trading on the "endorsement" given her by "Mr. Crookes and other Fellows of the Royal Society," which she naturally "improved" into that of "the Royal Society of England."

November 19

WILLIAM B. CARPENTER

#### Potential Energy

WILL you permit me to express a certain amount of scepticism as to the reality of Mr. O'Toole's troubles on this subject? That some statements made in the text-books quoted are not clear—that by an ingenious collocation of isolated passages from different authors some absurd conclusions may be drawn—we admit, but it may be doubted whether a *Publius* with the keen critical power of Mr. O'Toole would not be able to eliminate these errors or ambiguities by a reference to the context. In support of this position let us take the points raised by Mr. O'Toole in the order adopted by him,

A.—*Potential E., as meaning Energy in posse.*

The "doctors" quoted, with one exception, represent potential E.—not as energy *in posse*, but as kinetic energy *in posse*—a very different thing. Just as gold coin—though certainly not money *in posse*—may correctly be called silver coin (another form of money) *in posse*.

But it is said this name—and certain phrases employed by the doctors—imply that potential E. is "energy of about-to-surface motion, or that it does not perform work except through the resulting E. of motion." Mr. O'Toole is so distressed because poor *Publius* is susceptible to this impression, that I feel some hesitation in asking what is wrong in it? How can work be done without motion? How can the potential E. of a system change without a change in the configuration—i.e., motion of the system? Where is the mistake in the conception of potential E. continuously changing into kinetic energy, and this into work, as suggested by poor "P. M.," who was so summarily treated by this terrible O'Toole that I quake in my shoes as I think of my fate.

The exception mentioned above is an extract from Clerk Maxwell, which is certainly erroneous, and from which Mr. O'Toole gets a good deal of fun. We will not suggest that the addition of a single word would make the passage correct, for we should be told that text-books ought to be perfect. But it is only just to mention that the error occurs in an explanation of the name ; in the definition of the thing the error does not occur ; nay, it is expressly contradicted.

After this is it not unkind to condemn those doctors who drop the name "potential E." and replace it with such phrases as "E. of repose," &c., implying that the energy in question is not due to motion? By-the-by where is the bull in "passive energy"? and what is the "action" that may be confounded with kinetic energy?

[B.—Potential E. as meaning "Energy related to Potential Functions."]

The word Potential may be used in a second sense. This of itself is a trouble to Mr. O'Toole ; but—remembering that your readers may not sympathise with his undisguised antipathy to verbal skylarking—he hastens to add that the two meanings are not only heterogeneous but incompatible. "Surely there is no occasion to stop to prove this." Please do, Mr. O'Toole ; we should like to hear you prove something.

It may be noted that in this opinion and in paragraph 9 he appears to differ from Thomson and Tait. (See their definition of Potential, *Nat. Phil.*, vol. i., § 485.)

C.—Potential E. as meaning "Energy of Potency"

It appears from a foot-note that "potency" may mean a force. If so, it is strange that the O'Toole—who, throwing off his thin disguise, at the end of his letter undertakes the "duty" of a doctor, and tells us that potential E. should be the "energy of a force"—it is strange that Dr. O'Toole should object to the name on this ground.

But the remarks under this head are chiefly interesting, as indicating the *modus operandi* of our pseudo-Publius. He does not trouble to examine the definitions of "potential energy." He only looks for explanations of the word "potential." Finding scant material in the doctor's utterances, he resorts to his dictionary, hunts up the different meanings of "potential," adds to these their antitheses, and rends his phantoms to pieces. It is scarcely a parody upon his letter to say—we won't trouble about what a civil engineer is, but let us examine the meaning of *civil*. Now *civil* has—meanings : (A.) polite, (B.), &c. Therefore "civil E." means "polite E." and "civil E." used as a *distinguishing* title cannot mean anything else than this, that the other E. is unpolite E.

*As to the whereabouts of Potential Energy.*

We shall now pass from the perplexities connected with this unlucky name, "potential E.," to consider the behaviour of our teachers towards the thing itself." At last Mr. O'Toole will deign to discuss the definitions given by the doctors. Nay, he wanders away into an examination of such rash—but perhaps not inexcusable—phrases as "the potential E. of a raised weight," &c. The proper remedy for the troubles arising on this point is "to use words discreetly and consistently." But this is not sufficiently heroic. A local habitation must be found for this "potential E.," although it would seem as vain to inquire into the whereabouts of potential E. as into the whereabouts of Mr. O'Toole's scientific erudition. It is proposed to lodge this E. in the forces, and perhaps it won't do much harm, as we don't know where the forces are. It is proposed, moreover, to substitute "energy of tension" for "potential E." This done, the doctor's millennium will have come. Never mind about altering your conception of this kind of energy ; call it by another name ; give it a *weinichtwo* lodging. There will be no more "confusion about fundamental principles;" there will be no more slips of the pen or tongue ; there will be no more puzzled Publili ; and last, but not least, there will be no more O'Toolees to bother the doctors. Well may "verbal skylarking" be despised. What is beside such gigantic fun as this?

And yet I am sceptical. We started by hearing that it was "principally—though not entirely—the doctors who were to blame for this confusion about fundamental principles." Is this proved? Is not another cause indicated in the letter of of "E. G." (vol. xvii. p. 9)? And shall the doctors expect to be rightly understood when Dr. O'Toole's *amanuensis* admits (vol. xvi. p. 520) that Dr. O'Toole himself has been misapprehended upon almost every point by one reader at least?

Cirencester, November 13

H. W. LLOYD TANNER

Smell and Hearing in Moths

IN NATURE (vol. xvii. p. 72) your correspondent "E. H. K." observes : "J. C." seems to draw inferences that moths have not the power of smell, but have that of hearing. I feel quite certain they possess the former, but am in doubt about the latter. . . .

"With reference to the sound of the glass, is it not the quick motion of the hand which disturbs the moth?"

May I draw the attention of both your correspondents to some experiments of mine on this subject which were published in NATURE about year ago? These experiments, I remember, were quite sufficient to prove to me that moths have the power of hearing shrill notes ; and, until I read the query of "E. H. K." above quoted, I thought that my account of these experiments must have been equally conclusive to any one who read them. On now referring to that account, however, I find that I there omitted to state one of the experiments which was resorted to for the purpose of avoiding the possible objection which "E. H. K." now advances. This experiment was a very simple one, consisting merely in making a sudden shrill whistle with my mouth by drawing the breath inwards, so as not to disturb the air in the neighbourhood of the insect. The latter, however, always responded to this as to other sounds in the way described, although throughout the experiment I took care not to move any part of my body.

GEORGE J. ROMANES

It was because of my knowledge of facts like those named by "E. H. K." that I was surprised at the apparent inability of moths to smell ammonia. Being no physiologist, I ventured to draw no inferences ; but it occurred to me to wonder whether the sense of smell differs in kind with different organisations ; whether, for instance, some substances strongly odorous to us may be quite inodorous to insects, and *vice versa*.

As to the experiment on hearing, I do not think it was the movement of the hand which startled the moths. It may conceivably have been the vibration of their wings set up by the sound ; but the experiment can easily be repeated with variations by any one interested in the subject.

J. C.

Loughton

Meteorological Phenomenon

THIS morning at about a quarter before ten the sky here presented a most unusual appearance. The air was calm and the sun shining, but not brightly, through a slight veil of cirro-stratus. The sky was mostly covered with fibrous clouds of cirrus or cirro-stratus (I am not quite sure which I ought to call it), the fibres being quite parallel to each other, but in two different strata ; those of one stratum were approximately from north-east to south-west, those of the other from north-west to south-east—so that they seemed to cross each other like the threads of a woven fabric. I think the fibres from north-east to south-west were the highest, but am not quite sure, though it seemed the same to another who was looking on with me.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, November 25

OUR ASTRONOMICAL COLUMN

STELLAR SYSTEMS.—M. Flammarion, in various notes communicated recently to the Paris Academy of Sciences, has been drawing attention to stars which appear to be affected with a common proper motion, or a motion similar in amount and in its direction. Several of his cases, however, are by no means to be styled "Nouveaux systèmes Stellaires." Thus the large and uniform proper motions of the southern stars  $\zeta^1$  and  $\zeta^2$  Reticuli, to which he refers in the *Comptes Rendus* of November 5, were the subject of remark in NATURE, vol. xi. p. 328. That there was a probability of a common proper motion in these stars would be evident to any one who inspected the columns in the British Association Catalogue, published in 1845, but as Taylor had not observed them, and the comparison was consequently dependent upon Lacaille and Brisbane only, there was a possibility of mistake. The first confirmation of the large proper motion of the B.A.C. in  $\zeta^1$  was afforded in Jacob's "mean places of 1440 stars" from the Madras observations 1849-53, and

the earliest proof of a common translation in space was given by the same observer from the Madras observations 1853-58, which formed a part of vol. xviii. of the *Memoirs* of the Royal Astronomical Society. Not having seen any distinct reference to the very large and uniform motions of these stars in astronomical treatises, we adverted to them in NATURE as above.

Again, the common proper motions of Regulus and Lalande 19749, mentioned by M. Flammarion in the same communication have been long remarked. The same may be said in the case of 9 and 10 Ursae Majoris, one of the systems to which he refers in a paper presented to the Academy on November 12. Any one who has carefully utilised the very valuable fourteenth volume of the Dorpat observations must have been familiar with this case, and, we may add many similar ones, though the proper motions involved may be to smaller amount. This volume contains Mädler's laborious work upon 3222 of Bradley's stars, of which he gives positions reduced to 1850, and where all the catalogues available at the time and considered deserving of confidence were brought to bear. Not the least important feature in this work is the addition of two columns, not usually found in catalogues, containing the amount of secular proper motion in arc of great circle ( $r$ ) and the angular direction of this motion ( $\phi$ ) counted from north round by east to  $360^\circ$ . On p. 155 we have—

$$\begin{array}{lll} \text{For } 9 + \text{Ursae Majoris} & \dots & r = 52^\circ 5 \quad \phi = 238^\circ 9 \\ \text{“ } 10 & \dots & r = 52^\circ 6 \quad \phi = 238^\circ 5 \end{array}$$

But, as we have stated, other similar cases are readily detected by an inspection of these columns. For instance: in  $\gamma$  and 58 Tauri, distant  $35'$ , where  $r = 13'$ ,  $\phi = 97^\circ$ ; in 66 and 68 Draconis, distant  $43'$ ,  $r = 13^\circ 5$ ,  $\phi$  about  $69^\circ$  and for wider stars, in 26 and 34 Pegasi, distant  $4^\circ 25'$ , where  $r = 30'$ ,  $\phi = 84^\circ$ ; in  $\eta$  and 10 Arietis, distant  $5^\circ 11'$ ,  $r = 15^\circ 5$ ,  $\phi = 86^\circ$ , with other neighbouring stars, moving in nearly the same direction, and again in  $\mu$  and 54 Aquilæ, distant  $5^\circ 13'$ ,  $r = 27'$ ,  $\phi = 121^\circ$ . The list might be largely increased.

It is nevertheless to be expected that the researches which M. Flammarion is so industriously following up with respect to stellar systems may lead to a considerable addition to our knowledge of them, in cases which are not thus easily discovered from existing catalogues, particularly by determining the proper motions of stars, not yet submitted to such investigation.

**THE MINOR PLANETS.**—A letter from Prof. Watson, of Ann Arbor, U.S., to M. Yvon Villarceau, dated November 5, deranges the ordinal numbers of the small planets given in this column last week, from No. 175 onwards. It appears that on October 1 he discovered a planet 10m, which he duly notified by telegraph to the Smithsonian Institution, but by some unexplained circumstance the information was not transmitted by cable to the Observatory of Paris, as usual with such discoveries. Supposing this object to be really a new planet, it will be No. 175, and the subsequent discoveries mentioned last week will be on the same supposition, advanced a unit. Elements of No. 172 appear in *Astron. Nach.*, No. 2, 176, and of No. 176 in the *Paris Bulletin International* of November 25.

**THE CORDOBA OBSERVATORY.**—Within the last few days, Mr. John M. Thome, the zealous co-operator with Dr. B. A. Gould in the important astronomical work carried on for several years past at the Observatory of the Argentine Republic, has visited this country on his return to Cordoba from the United States. We have seen in his hands proofs of the charts of the Argentine "Uranometria," which are on a much larger scale than those of Argelander, Heis, and Behrmann. They have been engraved in New York. This work is expected to be soon published; also large lunar photographs taken at Cordoba. All the stars in the "Uranometria" have been meridionally observed.

#### CARL VON LITTRÖW

CARL LUDWIG VON LITTRÖW, whose death has been announced during the past week, was born at Kasan on July 18, 1811. His father, Joseph Johann von Littrow, the eminent astronomer, afterwards Director of the Imperial Observatory at Vienna, was at that time Professor of Astronomy in the University of Kasan, where he founded an observatory. The son was educated under the father's direction, and in 1831 was appointed assistant at the Observatory at Vienna, of which institution the elder Littrow had taken the superintendence in 1819, removing thence from Oefen. In 1835 he first appeared as an astronomical writer, having in that year published an account of Hell's Journey to Wardoe and of his Observations of the Transit of Venus in 1769 at that place, from the original day-books; also a History of the Discovery of General Gravitation, by Newton, and Treatises upon Comets, more especially on Halley's, which was then appearing. In 1839 he published at Stutgard a Celestial Atlas, and a work which in the Catalogue of the Pulkova Library is called a Translation of Airy's "Populäre physische Astronomie," by which is most probably intended the well-known Treatise on Gravitation published by the Astronomer-Royal in 1834, though elsewhere Littrow's work is stated to refer to the history of Astronomy during the early part of the nineteenth century, presented to the British Association in 1832.

In 1842 Carl von Littrow succeeded his father as Director of the Observatory of Vienna, and the establishment has continued in vigorous activity under his charge. He has principally devoted the energies of the Observatory to equatorial astronomy, following up with diligence the observations of comets and planets, and with the aid of able assistants determining their orbits. Some of the most complete cometary discussions have emanated from the Observatory of Vienna while under his charge. The *Annalen der Sternwarte in Wien*, have been continued, and valuable astronomical work is contained in them, as for instance in the first volume of the third series, which appeared in 1851, where we have the positions of the stars in Argelander's Northern Zone, reduced by Oeltzen to 1842, the epoch for which elements of reduction were given in the Bonn volume. Littrow was a frequent contributor to the publications of the Vienna Academy. In one of his memoirs—"Bahnäthen zwischen den periodischen gestirnen des Sonnensystems," printed in the *Sitzungsberichte* of the Academy für Jänner, 1854, he applied an original process of investigation of the points of nearest approach amongst the orbits of the small planets discovered up to that time, and the orbits of the periodical comets—a troublesome work in which mechanical aid was introduced; the result was the discovery of many close approximations of planets with planets, planets with comets, and of comets with comets; amongst the latter near approaches of Biela's comet to the orbit of Halley's in  $35^\circ$  and  $198^\circ$  heliocentric longitude. When interest was excited relative to the expected return of the comet of 1556, which at that period was supposed to have been previously observed in 1264, Littrow was the means of bringing to light an unknown treatise by Heller, which, with the chart of Fabricius, has allowed of a much improved determination of the orbit, and similarly he made known interesting particulars with reference to the remarkable observation by Steinhebel and Stark of a rapidly-moving black spot upon the sun's disc on February 12, 1820. Littrow was a constant contributor to the columns of the *Astronomische Nachrichten*. The names of Hornstein, Oeltzen, Weiss, Schulhof, and others are well known in connection with the work of the Vienna Observatory during Littrow's direction. His death occurred on the 16th inst.

Von Littrow's wife, Auguste Littrow-Bischoff, is one of the best known Austrian authoresses of the present time. The genial qualities of the astronomer and his wife made

them the centre of a large and admiring circle, and their residence was one of the most favourite gathering-places of the literary and scientific celebrities of Vienna.

#### BACTERIA<sup>1</sup>

**I**N a short paper communicated to the Royal Society at the close of last session, Prof. Tyndall did me the honour to criticise certain words reported to have been used by me at a meeting of the Association of Medical Officers of Health in January last. Although I am much indebted to him for the opportunity he has thus afforded me of discussing an important subject before this Society, I cannot refrain from expressing my regret that he should have thought it desirable to quote at length, and thus to place on permanent record in the Society's *Proceedings*, the expressions used on the occasion above mentioned. I regret this because these expressions occur in an abbreviated and incomplete abstract of a hastily prepared discourse not intended for publication.

As, however, I am well aware that Prof. Tyndall's purpose in his communication was not to criticise the language, but the erroneous views which the language appeared to him to contain, I shall make no further reference to the quotation; but shall regard it as the purpose of the present paper, first, to reply to the reasoning embodied in his last communication, and secondly to corroborate certain statements previously made by me, to which he has taken exception in the more extended memoir published in the 166th volume of the *Philosophical Transactions*.

It will be my first object to enable the Fellows of the Royal Society to judge how far the views I entertain differ from those which have been enunciated here and elsewhere by Prof. Tyndall. Biologists are much indebted to him for the new and accurately observed facts with which he has enlarged the basis of our knowledge, as well as for the admirable methods of research with which he has made us acquainted. As regards the general bearing of these facts on the doctrine of Abiogenesis, I imagine that we are entirely agreed. So far as I can make out, the difference between us relates chiefly to two subjects, namely, the sense in which I have employed the words "germ" and "structure," and the extent of the knowledge at present possessed by physiologists as to the structure and attributes of the germinal particles of *Bacteria*.

Although Dr. Tyndall, in the title of his paper, refers to my "views of ferment," yet as he makes no further allusion to them, I will content myself with stating that in the passage quoted, the first sentence (from the words "In defining" to the word "living") has nothing to do with the following sentences, having been placed in the position which it occupies in the quotation by the abstractor. The paragraph ought to begin with the words "Ten years ago."

Of the meaning which attached itself to the word "germ" in the days of Panspermism a correct idea may be formed from the following passage from M. Pasteur's well-known memoir "Sur les Corps organisés qui existent dans l'Atmosphère." "There exist," says he, "in the air a variable number of corpuscles, of which the form and structure indicate that they are organised. Their dimensions increase from extremely small diameters to one-hundredth of a millim., 1'5 hundredth of a millim., or even more. Some are spherical, others ovoid. They have more or less marked contours. Many are translucent, but others are opaque, with granulations in their interior. . . . I do not think it possible to affirm of one of these corpuscles that it is a spore, still less that it is the spore of a particular species of microphyte, or of another, that it is an egg or the egg of a particular microzoon. I confine myself to the declaration that the corpuscles are

evidently organised; that they resemble in every respect the germs of the lower organisms, and differ from each other so much in volume and structure that they unquestionably belong to very numerous species." Such are the "germs" of M. Pasteur, and such is the conception of a germ which was entertained by informed persons up to 1870, and is very generally entertained up to the present moment! It is obvious that these "corpuscles organisés" were, if they had any relation to *Bacteria*, not bacterium germs in Dr. Tyndall's sense, but "finished organisms," and yet it was of these that M. Pasteur said that it was "mathematically proved" that they were the originators of the organisms which are developed in albuminous liquids containing sugar, when exposed to the atmosphere.

With reference to the word "structure" I would point out that in the passage quoted from my lecture it is distinctly stated that the bacterial germ is endowed with structure in the molecular sense, but not in the anatomical sense. The meaning of the expression "anatomical structure" was, naturally, not defined, considering that the persons whom I was addressing might be supposed to be familiar with it. As, however, my failing to do so has apparently led to some uncertainty as to my meaning, I must, to avoid future misunderstandings, define more completely the difference between the two senses in which the word was used by me.

The anatomical sense of the word structure may be illustrated by referring to its synonyms, to the English words texture and tissue, to the Greek word *τεκτονία*, and to the German word *Gewebe*, from which two last the words in common use to designate the science of structure, viz., histology and *Gewebelehre* are made up. What I have asserted of the germinal particles of *Bacteria* is, that no evidence exists of their being endowed with that particular texture which forms the subject of the science of histology. In biological language there is a close relation between the words structure and organization, the one being an anatomical, the other a physiological term; either of these words signifies that an object to which it is applied consists of parts or structural elements, each of which is, or may be, an object of observation. As the observation is unaided or aided, the structure is said to be macroscopical or microscopical. The biologist cannot recognise ultra-microscopical structure or organisation except as matter of inference from observation, i.e., from observing either that other organisms, which there is reason to regard as similar to the object in respect of which structure is inferred, actually possess visible structure, or that the object can be seen to possess structure at a later period of its existence. As instances in which the existence of structure is inferred the following may be mentioned:—The protoplasm of a Rhizopod is admitted to have structure because, although none can be seen in the protoplasm itself, the complicated form of the calcareous shell which the protoplasm makes or models can be seen. By analogy therefore other organisms which are allied to the Rhizopod are inferred to have structure, and from these, or from similar cases, the inference is extended to all kinds of cells, with respect to which it is taught by physiologists that although, in certain cases, no parts can be distinguished, the living material of which they consist is nevertheless endowed with structure or organisation. Similarly, we assume, that a *Bacterium* possesses a more complicated structure than we can actually observe, because in other organisms which are allied with it by form and life history, such complications can be seen. Again, in all embryonal organs we admit the existence of structure before it can be seen, because in the course of

<sup>1</sup> "Remarks on the Attributes of the Germinal Particles of *Bacteria*, in reply to Prof. Tyndall," by J. Burdon-Sanderson, M.D., LL.D., F.R.S. Paper read at the Royal Society, November 22.

\* Before I became aware that the contaminating particles of water are ultra-microscopical I myself was engaged earnestly in hunting for germs both in water and air. The search has been continued by others up to a much later period. Those who desire information on the organised particles of the atmosphere will find the subject exhaustively treated by Dr. Douglas Cunningham in a report entitled "Microscopical Examinations of Air," lately issued by H. M. Indian Government.

respect each question are the result of a comparison up to the present "réalisés," "éléments," "s'explique," was the point of view of the anatomical and physiological differences between the two species. In the familiar comparison of the origin of the elephant with that of the mouse, in which the perfect anatomical similarity of the ova in the two species is contrasted with the enormous difference of the result, we should be justified in saying that the difference of development is the expression of structural difference between the primordium of the one and the primordium of the other; but inasmuch as it is not possible to indicate any anatomical distinction, it is understood that structural difference of another kind is meant, namely, difference of molecular constitution. In other words, we assume that the potential difference between the one and the other is dependent on an actual difference of molecular structure. Whether this is accompanied with an anatomical difference, such as we might expect to be able to see if we had more perfect instruments, we do not know.

From the moment that it is understood that the word structure means anatomical structure, the argument used by Dr. Tyndall loses its relevance. After referring to the "germ limit," he says, "some of those particles" (by which, I presume, is meant atmospheric particles) "develop into globular *Bacteria*, some into rod-shaped *Bacteria*, some into long flexible filaments, some into impetuously moving organisms, and some into organisms without motion. One particle will emerge as a *Bacillus anthracis*, which produces deadly splenic fever; another will develop into a *Bacterium*, the spores of which are not to be microscopically distinguished from those of the former organism; and yet these undistinguishable spores are absolutely powerless to produce the disorder which *Bacillus anthracis* never fails to produce. It is not to be imagined that particles which, on development, emerge in organisms so different from each other, possess no structural differences. But if they possess structural differences they must possess the thing differentiated, viz., structure itself." Throughout this passage it is evident that it is not anatomical but molecular structure that is referred to. In the other passages relating to the subject, I venture to think that Dr. Tyndall has overlooked the distinction made by me between anatomical organisation and molecular structure. When, for example, he speaks of "germ structure" in the passage quoted from his Liverpool Address, he evidently refers to molecular structure exclusively, for he gives ice as his first example, and argues that as ice possesses structure so do atmospheric germs—a proposition which I should not have thought of questioning.

The experimental evidence which we have before us goes to prove that in all the known cases in which *Bacteria* appear to originate *de novo*—that is to say in liquids which are at the moment of their origin absolutely free from living *Bacteria*—they really originate from "particles great or small," which particles are therefore germs in the sense in which that word is used by Prof. Tyndall. To illustrate the views I myself entertain, and always have entertained on this question, I need only refer to my paper on the origin of *Bacteria*, published in 1871. The experiments made by me at that time brought to light the then new fact, now become old by familiarity, that all exposed aqueous liquids, even when absolutely free from visible particles, and all moist surfaces, are contaminated and exhibit a power of communicating their contamination to other liquids. As regards water and aqueous liquids in general, I insisted on the "particulate" nature of the contaminating agent, and coined for the purpose the adjective I have just employed (which has been since adopted by other writers), at the same time pointing out

that the particles in question were ultra-microscopic, and consequently that their existence was matter of inference as distinguished from direct observation. Dr. Tyndall has demonstrated by the experiments to which I have already alluded, that the ordinary air also contains germinal particles of ultra-microscopic minuteness. Of the completeness and conclusiveness of those experiments I have only to express the admiration which I, in common with all others whose studies have brought them into relation with the subject, entertain. That such particles exist there can be no question; but of their size, structural attributes, or mode of development, we know nothing.

Prof. Tyndall, I am sure by inadvertence, has accused me of assuming that there is some relation between the limit of microscopical visibility and what he calls the molecular limit, by which I presume to be meant the size of the largest molecule. Nothing that I have said or written could justify such a supposition. My contention is not that the particles in question are of any size which can be specified, but, on the contrary, that we are not in a position to form any conclusion as to their size, excepting that they are so small as to be beyond the reach of observation. Dr. Tyndall has taught us, first, that the optical effects observed when a beam of light passes through a particulate atmosphere are such as could only be produced by light-scattering particles of extreme minuteness; and, secondly, that by subsidence these particles disappear, and that the contaminating property of the atmosphere disappears with them. He has thus approximately determined for us the upper limit of magnitude, but leaves us uncertain as to the lower; for we have no evidence that the particles which render the atmosphere opalescent to the beam of the electric lamp may not be many times larger than those which render it germinative. Consequently, the fact that the air may be rendered sterile by subsidence, while affording the most conclusive proof that germinal matter is not gaseous, leaves us without information as to the size of the particles of which it consists.

Of each germinal particle, whether inhabiting an aqueous liquid or suspended in the atmosphere, it can be asserted that under conditions which occur so frequently that they may be spoken of as general (viz., moisture, a suitable temperature, and the presence of dead proteid matter, otherwise called organic impurity), it produces an organism. If, for the sake of clearness, we call the particle *a* and the organism to which it gives rise *A*, then what is known about the matter amounts to no more than this, that the existence of *A* was preceded by the existence of *a*. With respect to *A* we know, by direct observation, that it is an organic structure; but inasmuch as we know absolutely nothing as to the size and form of *a*, we cannot even state that it is transformed into *A*, much less can we say anything as to the process of transformation.

Considering that it is admitted on all hands that there exist in ordinary air particles which are potentially germs, it might at first sight appear needless to inquire whether or not this fact is to be regarded as carrying with it the admission that they must necessarily possess the other attributes of organised structure. Very little consideration, however, is requisite in order to become convinced that this question stands in relation with another of fundamental importance in biology—that, namely, of the molecular structure of living material.<sup>1</sup> It is not necessary for my present purpose to do more than to indicate the nature of this relation. As regards every form of living matter, it may be stated that, quite irrespectively of its morphological characteristics, which, as we have seen,

<sup>1</sup> The reader who is interested in this subject will find it discussed with great ingenuity by Prof. Pflüger, in his paper "Über die physiologische Verbrennung in den lebendigen Organismen," *Pflüger's Archiv*, vol. x. p. 300.

must be learnt by the application of the various methods of visual observation at our disposal, it possesses molecular structure peculiar to itself. We are certain of this, because the chemical processes of which life is made up are peculiar, that is, such as occur only in connection with living material. Even the simplest instance that we can mention, that of the elevation of dead albumin into living (a process which in the case now before us must represent the very earliest step in the climax of development) is at the present moment beyond the reach of investigation; for as yet we are only beginning to know something about the constitution of non-living proteids. But this want of knowledge of the nature of the difference between living and non-living material in no wise impairs the conviction which exists in our minds that the difference is one of molecular structure.

The sum of the preceding paragraphs may be stated in few words. Wherever those chemical processes go on, which we collectively designate as life, we are in the habit of assuming the existence of anatomical structure. The two things, however, although concomitant, are not the same; for while anatomical structure cannot come into existence without the simultaneous or antecedent existence of the kind of molecular structure which is peculiar to living material, the proof is at present wanting that the vital molecular structure may not precede the anatomical. At the same time it must be carefully borne in mind that there is no evidence of the contrary. It is sufficient for my purpose to have shown that the existence of organised particles endowed with anatomical structure in the "atmospheric dust" has not been proved. I do not dispute its probability.

Before leaving this subject I may be permitted to add a word as to the bearing of this discussion on a question which, to myself, is of special interest—that of *contagium vivum*. According to the view which these words are understood to express, the morbid material by which a contagious disease is communicated from a diseased to a healthy person consists of minute organisms, called "disease-germs." In order that any particle may be rightly termed a disease-germ two things must be proved concerning it, viz., first, that it is a living organism; secondly, that if it finds its way into the body of a healthy human being, or of an animal it will produce the disease of which it is the germ. Now there is only one disease affecting the higher animals in respect of which anything of this kind has been proved, and that is splenic fever of cattle. In other words, there is but one case in which the existence of a disease-germ has been established.

Comparing such a germ with the germinal particles we have been discussing, we see that there is but little analogy between them, for, first, the latter are not known to be organised; secondly, they have no power of producing disease; for it has been found by experiment that ordinary *Bacteria* may be introduced into the circulating blood of healthy animals in considerable quantities without producing any disturbance of health. So long as we ourselves are healthy, we have no reason to apprehend any danger from the morbid action of atmospheric dust, except in so far as it can be shown to have derived ineffectiveness from some particular source of miasma or contagium.

I now proceed to the second part of my communication, which relates to Prof. Tyndall's serious, but most courteously-expressed, criticisms of my experiments on spontaneous generation.<sup>1</sup>

<sup>1</sup> The expressions referred to are the following:—"I have worked with infusions of precisely the same specific gravity as those employed by Dr. Bastian. This I was especially careful to do in relation to the experiments described and vouch'd for. I fear inadvertently, by Dr. Burdon-Sanderson, in vol. vii, p. 180 of NATURE. It will there be seen that though failure attended some of his efforts, Dr. Bastian did satisfy Dr. Sanderson that in boiled and hermetically-sealed flasks *Bacteria* sometimes appear in swarms. With purely liquid infusions I have vainly sought to reproduce the evidence which convinced Dr. Sanderson. . . . I am therefore compelled to conclude that Dr. Sanderson has lent the authority of his name to results whose antecedents he had not sufficiently examined." Phil. Trans., vol. clxvi.

The fact that Dr. Tyndall blames me for incautiously vouching for is, "that in boiled and hermetically-sealed flasks *Bacteria* sometimes appear in swarms." From multiplied experiments he concludes that this is not true, and infers that I who vouch'd for it was incautious. The paper referred to was one in which I, as a bystander, gave an account of certain experiments which Dr. Bastian performed in my presence. So far as relates to the fact above quoted, these experiments were, to my mind, absolutely conclusive; but inasmuch as I was unable to admit with Dr. Bastian that they afforded any proof of spontaneous generation, I followed them as soon as practicable by a series of experiments (NATURE, vol. viii, p. 141) (the only ones which I myself ever made on this subject), in which I tested the influence of two new conditions, viz., of prolonged exposure to the temperature of ebullition, and of exposure for short periods to temperatures above that of ebullition at ordinary pressure. The experiments accordingly consisted of two series, in the first of which a number of retorts or flasks charged with the turnip-cheese liquid, i.e. with neutralised infusion of turnip of the specific gravity 1017, to which a pinch of pounded cheese had been added, and sealed hermetically while boiling, were, after they had been so prepared, subjected to the temperature of ebullition for longer or shorter periods. In the second series the period of ebullition was the same in all cases, but the temperature was varied by varying the pressure at which ebullition took place.

The conclusion arrived at, as expressed in the final paragraph of the paper, was, that in the case of the turnip-cheese liquid, the proneness of the liquid to produce *Bacteria* can be diminished either by increasing the temperature employed to sterilise it, or if the ordinary temperature of ebullition be used, by prolonging its duration.

I did not think it necessary after 1873 to occupy myself further with the subject for two reasons, first, that I had accomplished my object, which was to show that as a ground for believing in spontaneous generation the turnip-cheese experiment was a failure; but secondly, and principally, because in the meantime the subject had been taken up by the most competent living observers, who had in every particular confirmed the accuracy of my results. I conclude this paper by referring shortly to some of these researches.

The first was made by P. Samuelson under the direction of Prof. Pflüger<sup>1</sup> in 1873. Its purpose was to ascertain whether it is true that certain liquids can be boiled for ten minutes without being sterilized, and secondly, to determine the influence of prolonged periods of exposure. The flasks employed were charged with the neutral turnip-cheese liquid, and sealed while boiling in the way already described. Some were subjected to the temperature of ebullition for ten minutes, the rest for an hour, the result being that whereas those heated for the longer periods remained without exception barren, an exposure of only ten minutes was followed, in the majority of cases, by an abundant development of *Bacteria*.<sup>2</sup> At about the same period a similar series of experiments was made under the direction of Prof. Hoppe-Seyler at Strasburg. The results were essentially the same.<sup>3</sup>

p. 57. In the abstract of a lecture delivered at the Royal Institution, January 21, 1876, similar words occur, as also in a letter to NATURE, dated February 27, 1876, in which Dr. Tyndall, after remarking that the experiments of Dr. Bastian, witnessed by me, were too scanty and too little in harmony with each other to bear an inference, suggests that I should repeat them.

<sup>1</sup> "Ueber Abiogenesis," von Paul Samuelson aus Königsberg, Pflüger's Archiv, vol. viii, p. 277. The paper is designated as a report of experiments made "im Auftrag und unter der Leitung des Geh.-Rath Prof. Pflüger." I refer in the text only to those experiments which were virtually repetitions of my own. The research actually extended over a wider field.

<sup>2</sup> "Als Resultat dieser Versuchsreihe, ergab sich eine massenhafte Entwicklung von Bakterien in den meisten nur 10 Minuten lang gekochten Flüssigkeitsmengen nach 3-4 Tagen" (loc. cit. p. 283).

<sup>3</sup> "Ueber die Abiogenesis Huiizinga's," von Felix Putzeys aus Lüttich (aus dem chemisch-physiologischen Laboratorium des Herrn Prof. Hoppe-Seyler), Pflüger's Archiv, vol. ix, p. 391. In a note appended by Prof. Hoppe-Seyler to this paper he states that he has recommended its publica-

During the next year the second question which I had attempted to solve, viz., the influence of temperatures above  $100^{\circ}$  C., was taken up with much greater completeness by Prof. Gscheidlen, of Breslau.<sup>1</sup> After a *résumé* of the proofs already given by his predecessors, that certain fluids are not sterilised by boiling; and, secondly, that as means of sterilising such liquids the action of prolonged exposure and that of increased temperature may be regarded as complementary to each other, he proceeds to relate his own researches, the purpose of which was rather to fill up defects in the evidence than to establish new conclusions.

The flasks employed were capable of containing 100 cub. centims. (three and a half oz.); they were charged in the usual way with the turnip-cheese liquid, and exposed for short periods in chloride of calcium baths, of which the strengths were carefully adjusted so as to obtain the requisite temperatures. It was thereby definitely proved that whereas the germinal matter of *Bacteria* can stand a temperature of  $100^{\circ}$  for five or ten minutes it is destroyed by temperatures varying from  $105^{\circ}$  to  $110^{\circ}$ .<sup>2</sup>

In an appendix to my first paper, published in NATURE in the autumn of 1873, I showed that the solution of diffusible proteids and carbo-hydrates employed by Prof. Huizinga, of Groningen, in the first of the valuable series of experiments<sup>3</sup> published by him, relating to the subject of spontaneous generation, require a temperature above that of ebullition under ordinary pressure to sterilise them. This observation has since been established by Prof. Huizinga himself on the basis of very carefully made experiments,<sup>4</sup> by which he has proved at the same time that the liquids in question are rendered completely incapable of producing *Bacteria* without extrinsic contamination by exposing them to higher temperature. The only points of difference between us, either as regards method or result, are, first, that the sterilisation limit (*Grenze zur Bacterienerzeugung*) fixed by me was too low—the true limit being  $110^{\circ}$  C.—and secondly, that the experiments from which I had inferred that the liquids in question had been sterilised at lower temperatures than this were, in Prof. Huizinga's opinion, rendered inconclusive by the fact that my flasks were sealed hermetically.

Notwithstanding that the results obtained were mere confirmations of those of former observers: adding "für den wissenschaftlichen Fortschritt hat nicht die Priorität des einen oder des anderen Beobachters, wohl aber die Zahl, Mannigfaltigkeit, und Zuverlässigkeit der Beobachtungen eine hohe Wichtigkeit."

<sup>1</sup> "Ueber die Abiogenesis Huizinga's," von Richard Gscheidlen, *Pflüger's Archiv*, vol. ix. p. 163.

<sup>2</sup> "Es folgt aus den eben angegebenen Versuchen, nach meiner Meinung, dass in Huizinga's Gemengen die Bakterien einer Temperatur von  $110^{\circ}$  to Minuten lang zu widerstehen vermögen, nicht aber über von  $100^{\circ}$ - $110^{\circ}$  in eingeschmolzenem U-lasrohr während der nämlichen Zeit" (*loc. cit.* p. 167). Here the author clearly fails to make the necessary distinction between *Bacteria* (which, as is well known, lose their vitality at a much lower temperature) and the material out of which they spring. The mixture referred to were either the cheese and turnip liquid or solutions containing peptones and grape sugar, to be immediately referred to. As affording an elegant demonstration that in the turnip-cheese liquid it is the cheese and not any other constituent which contains the resistant element, the following form of experiment is worthy of notice:—A tube *a* drawn out and closed at both ends is fused into the open mouth of a second tube *b*, of which the opposite end is drawn out and closed in a similar manner. In this way a compound tube is formed which is divided by a conical septum into two chambers *a* and *b*. A small knob of glass having been previously introduced into the chamber *b*, the septum can be easily broken by shaking the tube. With tubes so prepared, two experiments are made. In Experiment *a*, compartment *a* is charged with infusion of cheese sealed, and then exposed to a temperature of  $110^{\circ}$  before it is united to the compartment *b*. In like manner *b* is charged with neutral decoction of turnip, so that when the compound tube is complete it contains cheese in one compartment, turnip in the other. If, after boiling for ten minutes, it is placed in the warm chamber its contents remain barreled. In Experiment *b* the experiment is varied by simply omitting the preliminary heating of *a*. The compound tube is boiled as before, but now its contents promptly give evidence that the conditions are present for an abundant development of *Bacteria*.

<sup>3</sup> Prof. Huizinga's papers on the Question of Abiogenesis are four in number. The references are as follows:—*Pflüger's Archiv*, vol. vii. p. 225, vol. viii. pp. 180, 551; vol. x. p. 62.

<sup>4</sup> The solution employed in these experiments was neutral, and contained, in addition to the requisite inorganic salts, 2 per cent. of grape sugar, 0.3 per cent. of soluble starch, 0.3 per cent. of peptones, and 1 per cent. of ammonic tartate. As in my experiments, the flasks were heated in a Papin's pot, of which the temperature was  $102^{\circ}$  C. Even after half an hour's exposure to this temperature all the flasks became in two or three days "stark trüb und voll Bakterien," third paper, p. 555, January, 1874.

cally, whereas in his exchange of air was allowed to take place during the period of incubation, through a septum of porous porcelain. To this last objection I might perhaps have thought it my duty to answer, had it not been shown by the subsequent researches of Gscheidlen to have no bearing on the question at issue. As regards the limit of sterilisation I can entertain no doubt as to the accuracy of Huizinga's measurements, and am quite willing to accept  $108^{\circ}$  C. as the lowest temperature which could be safely employed under the conditions laid down by him.

It will be understood that in bringing these facts before the Society my only purpose is to show, as I trust I have done conclusively, that the statements which Dr. Tyndall in 1876 characterised as cautious, and which he virtually invited me to retract, had been two years before confirmed in every particular by experimenters of acknowledged competence.

#### DIFFUSION FIGURES IN LIQUIDS<sup>1</sup>

IN making some experiments on the mixture of liquids I entering into another liquid at the extremity of a tube of small diameter, a phenomenon presented itself which attracted my attention as both new and singular. A certain quantity of coloured alcohol, remaining in suspension in the centre of a body of water, assumed, by spreading gradually out, a form resembling that of a shrub having its trunk and its branches terminated by leaf-like expansions. I sought to reproduce the pheno-

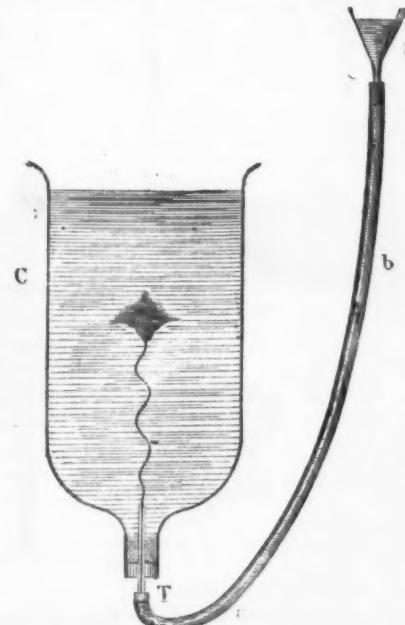


FIG. 1.—Apparatus of Prof. Martini.

menon, believing at first that this mode of diffusion was purely accidental; but the phenomenon always recurring very nearly in the same manner, I devised a mode of experimenting which enabled me to study it more advantageously.

C (Fig. 1) is a sort of cylindrical funnel of glass, to the neck of which is fitted a small capillary thermometrical tube T, about eight centimetres long. The capillary tube communicates by means of a caoutchouc tube a b, with a

<sup>1</sup> From an article in *La Nature* by Prof. Tito Martini, of Venice.

small funnel 1, which may be raised or lowered at pleasure by means of its support. Pour into 1 a certain quantity of alcohol coloured say with a red solution of aniline. The liquid will traverse the capillary tube, from which it will flow unless prevented by compressing the india-rubber tube with a small pincers. This being done, fill with water the vessel C about three-fourths full; then by means of a funnel whose lower extremity reaches a little below the middle of the water, introduce a liquid denser than water, a concentrated solution of sea-salt or a thick syrup, until the vessel is filled up. Sulphuric acid may also be used, and in that case a less volume of liquid will suffice.

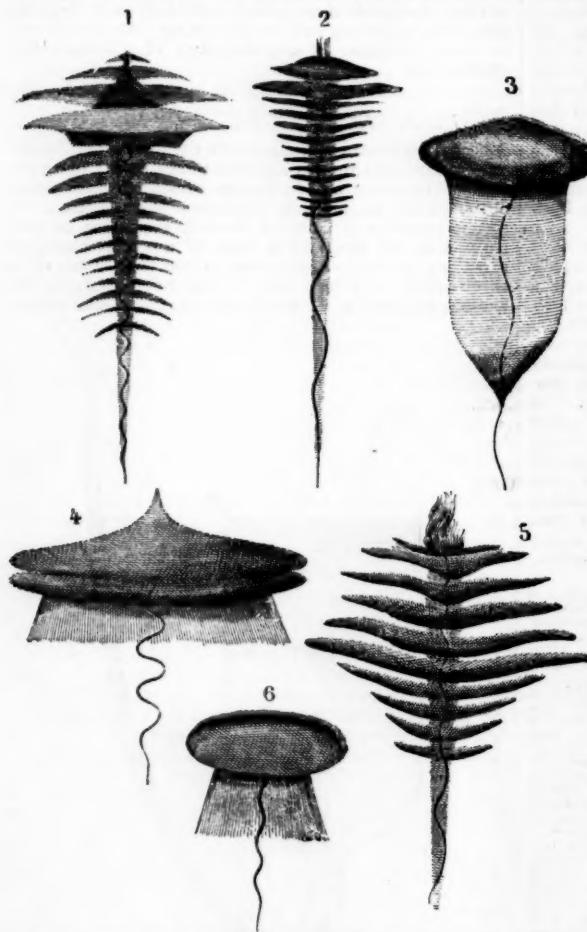


FIG. 2.—Experiments of Prof. Martini on the diffusion of coloured liquids in a sirupy liquid.

The liquid more dense than water will collect at the bottom of C; and there will thus be two layers of liquid superposed, the exact separation of which may be observed after being allowed to stand for an hour. If at the end of that time we raise the funnel 1 to a suitable height and relieve the pincers which compress the tube ab, the coloured alcohol which flows from the extremity of the capillary tube will enter the liquid in the vessel C, forming an ascending vein which usually has a spiral form. The alcoholic vein traverses the thickest layers of the liquid and is stopped at the boundary which separates the denser from the less

dense part which floats above. At the point where the column of coloured alcohol is arrested, it will be seen to agglomerate into a mass at first formless; but, gradually, that mass elongates and extends, then is seen to throw out fluid threads in the form of foliage, sometimes similar to the petals of a flower, sometimes analogous to the leaves of a tree. After an hour the coloured alcohol has assumed a stable and regular figure. That figure varies in form with the liquids employed; it sometimes resembles a flower, sometimes a shrub, and sometimes it takes the form of a parasol of bright and vaporous colours, which add to its beauty.

The figure, so far as its form is concerned, attains its maximum of development three hours or more after the fluid vein begins to flow; but after that time the leafy expansions dilate more and more, and approach each other so as to form a mass of continuous layers, which remain suspended in the midst of the liquid. This happens even when the inflow has been arrested, either by applying the pincers to the india-rubber tube, or even by lowering suitably the funnel, 1. It should also be remarked that around the vein of ascending liquid there very often forms a very fine tube, which assumes the aspect of the stalk of the flower, or rather the trunk of the liquid shrub; from different points of that stalk expansions in the form of leaves will be seen to proceed.

In order that the experiments I have devised may be successful, the tube through which the coloured liquid enters the vessel ought to be capillary, the flow ought to be gentle, and the apparatus maintained in a state of complete rest. It is necessary, moreover, to be careful first to expel the air from the india-rubber tube, since air-bubbles disturb the formation of the phenomenon. The following is a succinct *résumé* of some of the results I have obtained with different liquids:—

*Colours of Aniline Solution.*—I made use of aniline red, brown, green, and violet, dissolved in alcohol, being careful that the solution was not too concentrated. The forms obtained in sugared, salted, and acidulated water, are those represented in Fig. 2, Nos. 1 and 2. The figures obtained resemble, as will be seen, leaf-like expansions; the ramifications are turned downwards in sugared water (No. 1); in salt water, on the contrary, they are always raised, and at the commencement even more so than is shown in the figure. When acidulated water is used, the aniline colours are modified by the action of sulphuric acid; the green becomes pale yellow, the red becomes brown, and the violet acquires a beautiful green colour; but in all cases the shrub-like figure No. 2 is formed with perfect regularity.

*Litmus. Aqueous Solution.*—With this solution we obtain in acidulated water the figure represented in No. 3 (Fig. 2), which resembles a small parasol. Looked at from above, it has the aspect of a disc from the periphery of which proceed many equidistant rays very close to

each other. In the salt water the same aqueous solution gives a different figure. In general, when aqueous solutions are employed to form the figures a space of time is required longer than that which is necessary in the case of alcoholic solutions.

*Alcoholic Solution.*—With this solution there are formed in salt or sugared water, figures analogous to Nos. 1 and 2; in acidulated water there is produced a shrubby appearance similar to No. 2.

*Lake.*—The aqueous solution of lake forms in salt water a figure similar to that of No. 4; in acidulated

water Fig. 3 is produced, but more delicate and more regular than that obtained with litmus.

*Azure Blue.*—The aqueous and alcoholic solutions of azure-blue or pearl form figures similar to those already described. In acidulated water we obtain a very regular spheroidal nucleus of a very dark blue, surrounded by a spheroidal layer with an inferior stem (No. 6).

*Cochineal.*—The aqueous solution forms in acidulated water the figure No. 3, regular, like that of litmus and of lake. In salt water, cochineal, not being soluble, is precipitated and the phenomenon is not produced.

*Iodine.*—The alcoholic tincture of iodine forms, in sugared, salt, or acidulated water, beautiful figures almost identical with those of the colours of the aniline solution.

*Bichromate of Potash.*—To make the experiments with bichromate of potash succeed I changed the arrangement of the experiment on account of the very great density of the solution in comparison with the density of water. I fill the vessel in the usual manner, then I place above the vessel a small funnel, fitted with a capillary tube which partly enters the liquid. The aqueous solution of bichromate of potash being poured into the small funnel, flows out, forming a small descending spiral, which usually is arrested in the division between the more and less dense parts of the liquid. In acidulated or salt water two very beautiful figures are formed resembling those of Nos. 2 and 5, but reversed.

The various experiments described above have been repeated several times for each colour, and I have always obtained the same results. This persistence of form shows that the phenomenon is regulated by a law which I shall seek to discover. I believe I may conclude from these first attempts that the form of the figure depends on the liquid in which the colour is dissolved, more than on the colour itself. By employing other acids and other salts, not such, however, as precipitate the colour, it is probable that other figures would be obtained.

#### TRACES OF EARLY MAN IN JAPAN

SO much interest is felt in the origin of the Japanese, that any information regarding earlier races in Japan will interest the readers of NATURE.

The discovery and examination of a genuine klockkenmoedding, or shell heap, enables me to give positive evidences regarding a prehistoric race who occupied this island. Whether autochthonous or not it would of course be impossible to say. On my first ride to Tokio, in June of this year, I observed, from the car window, near a station called Omori, a fine section of a shell heap, which was recognised as such at once, from its resemblance to those I had often studied along the coast of New England. On September 16, accompanied by Messrs. Matsumura, Matsura, and Sasaki, three intelligent Japanese students, I made an examination of it, and a few days afterwards, in company with Dr. David Murray, Superintendent of Public Instruction, and Mr. Vukuyo, with two coolies to do the heavy digging, made an exhaustive exploration of it.

The deposit is composed of shells of various genera, such as *Vusus*, *Eburna*, *Turbo*, *Pyrula*, *Arca*, *Pecten*, *Cardium*, two strongly marked species of *Ostrea*, and curiously enough, *Mya arenaria*, not to be distinguished from the New England form, as well as other genera. These shells, so far as I know, still live in the Bay of Yedo. The heap is about 200 feet wide, and varies from a foot to five or six feet in thickness, with a deposit of earth above, at least three feet in thickness. It is now nearly half a mile from the shore of the Bay, though in accordance with the usual position of these heaps in other parts of the world, it must have been formed near the shore, and this fact indicates a considerable elevation of the land since the deposits were made. I may add that other

evidences of a geological nature indicate a wide-spread upheaval in past times.

The peculiarities of the typical shell-heap, such as fragments of bones, rough implements worked out of horn, and pieces of pottery, are all here. The heap, however, is marked by certain features which render it peculiar.

First, the immense quantity of pottery and its diversity of ornamentation, some of it extremely ornate, but very rude.

Second, the absence of bone-implements, the few found—eight or ten in number—being of horn, with the exception of an arrow-head of diminutive proportions, made of the tusk of a wild boar. All the implements are very simple; two of them are like blunt bone awls, with the end very obtuse, and a constriction worked around the end. Another one is made from the natural termination of a deer's antler. A few fragments of horn were found which had been cut off at the ends.

Third, the entire absence of flint flakes, or stone implements of any kind, if we except a small stone adze found near the top of the heap, and made out of a soft sandstone. The frequent occurrence of isolated tusks of the wild boar would seem to indicate that these teeth were used for implements, and one piece of antler, having a hole in the end, is worked in the form of a rude handle. By far the most common bones found were those of the deer and wild boar, and curiously enough Steenstrup shows the same proportion in the Danish shell heaps. No human bones have yet been found.

An analysis of the red pigment found on some of the pottery shows it to be cinnabar. With its removal from the shore, its elevation above the level of the sea, the absence of stone implements, and the great thickness of the earth deposits above, we have reasons for believing that the deposit is of high antiquity.

Through the intelligent interest manifested by Mr. Kato and Mr. Hamao, Director and Vice-Director of the Imperial University of Tokio, every facility for a thorough investigation of these deposits will be given me.

Tokio, Japan, September 21 EDWARD S. MORSE

#### NOTES

IT is proposed to hold the next annual meeting of the Association for the Improvement of Geometrical Teaching (under the presidency of Dr. Hirst) at University College, Gower Street, on January 11, 1878, at 10.30 A.M. Four resolutions are to be submitted to the Association:—1. That in the opinion of this Association it is both reasonable and expedient that candidates at all examinations in elementary geometry should be required to give evidence of such ability as is necessary for the solving of easy geometrical exercises; and that the secretaries of the Association be instructed to send a copy of this resolution to the leading examining bodies of the country. The other resolutions relate to the proposed formation of sub-committees for drawing up a syllabus of (1) Solid Geometry, (2) Higher Plane Geometry (Transversals, Projection, &c.), (3) Geometrical Conics. It may be in the recollection of our readers that the report of the British Association Committee (in 1876, published at the time in NATURE) was highly favourable to the work of this Association.

THE dissection of the Berlin gorilla was performed last week by Prof. Virchow and Prof. Hartmann in the presence of several prominent Berlin physicians, and it was ascertained that the sudden death of the animal was caused by acute inflammation of the bowels, the same disease which carries off young children so rapidly. The dissection explains the cause of his previous illnesses and supplies valuable information with regard to the treatment of anthropoidal apes. The button of a glove, iron wire, and pins were found in Pongo's stomach.

DURING the past week the Emperor of Germany received a deputation of the members of the German Expedition for observing the transit of Venus, who presented him with a handsomely-mounted album containing copies of all the photographs taken during the transit.

BERN celebrates on December 12 the 100th anniversary of the death of its famous citizen, Albert Haller, who was equally renowned as physiologist, botanist, and poet.

THE New York *Nation* informs us that news has been received of the death of the Rev. James Orton, professor of natural history at Vassar College, and well known as the author of "Comparative Zoology" and "The Andes and the Amazon." Prof. Orton made his first expedition to South America in 1867, crossing the Andes eastward from Peru, and descending the Napo to the Marañon. His second expedition in 1873 was the reverse of the former one, beginning with the ascent of the Amazon. He was on his way home from a third expedition when he died, September 25, on board a small schooner on Lake Titicaca. He was greatly esteemed by all who knew him.

THE New York *Tribune* states that Mr. Edison, the inventor of many improvements in telegraphy, is hard at work in the endeavour to make the telephone record the sounds it transmits. His apparatus at present consists chiefly of a steel point attached to the disk of a telephone and pressing lightly on a strip of paper passed beneath the point at a uniform rate. The vibrations of the disk are thus recorded, and can be translated. Mr. Edison has already achieved some success in this attempt, but as yet finds difficulty with the more delicate vibrations. The invention suggests an ultimate possibility of recording a speech at a distance, verbatim, without the need of shorthand.

NOT one of the designs sent in competition for the monument to Spinoza at the Hague has satisfied the judges. A new term for receiving designs will therefore be fixed.

ANOTHER letter from Mr. Stanley appears in the *Telegraph* of Thursday last, in which he gives many interesting details of his journey down the Lualaba-Congo, but does not add essentially to what we already know from previous letters. It will be well at present to rest satisfied with the fact that he has solved a great geographical problem; discussion will be appropriate and to some purpose when we are in possession of the full details. In the December number of Petermann's *Mittheilungen* that keen geographer discusses the bearings of Stanley's discovery, and on the basis of the earlier letters identifies the Lualaba-Congo with the discoveries of Browne, Barth, Nachtigal, and Schweinfurth; but on the map which accompanies the paper he carries the great river north to about 4° N. lat. In a postscript on Stanley's own map Dr. Petermann seems to think that his identifications may require modification. Dr. Petermann cannot find terms strong enough in which to speak of the merit of Stanley's work. He calls him "the Bismarck of African exploration," who has united the *disjecta membra* of previous explorations as Bismarck has made one great empire out of a number of isolated states. He is evidently inclined to place Stanley alongside of Columbus.

THE December number of Petermann's *Mittheilungen* contains a long paper on the Iquique earthquake of May 9 last, in which much valuable data are given on the earthquake and on the wave which was simultaneous with it over so wide a stretch of the Pacific Ocean.

THE *Daily News* correspondent at Rome writes that no news has arrived there as to the death of the African explorer, the Marquess Antinori, the inference being that he is still alive. A long letter has been received by the Italian Geographical Society from Signor Matteucci who, with Signor Gessi, is bound for Inner Africa; the two expect to be in Khartoum in the

beginning of December. They were splendidly equipped before leaving Italy.

DR. SCHWEINFURTH, the celebrated African traveller, who has been staying at Berlin since the beginning of August, will shortly return to Africa, as he finds that the European climate no longer agrees with his health. At present he has left Berlin for Weimar.

AT the Geographical Society, on Monday night, Commander Musters, R.N., read a paper on Bolivia, in which he gave much valuable information about a country, its products and its people, about which we are extremely ignorant. Commander Musters lived in the country for a considerable time. Mr. Clements R. Markham read a paper on the still unexplored parts of South America. The facts are we are almost as ignorant of Central South America as, until recently, we were of Central Africa, and there is here a practically virgin field for a second Stanley, if not indeed for Stanley himself.

IN a recent number we referred to the preparations which are being made for Prof. Nordenskjöld's expedition to the Arctic regions next summer. The *Handels och Sjöfarts Tidning* of Gothenburg publishes further details, giving the plan of the expedition as presented to the King of Sweden by Prof. Nordenskjöld. We now learn that the steamer *Vega* is being fitted up at the royal wharves of Carlshafen, and will take provisions for two years. The Professor intends to leave at the beginning of July next, and his staff will consist of four scientific men besides himself, four Norwegian sailors who are well acquainted with the Arctic Sea, a ship's officer, eighteen marines, and a ship's doctor. The first halt will be made at the mouth of the Yenisei River; then the expedition will proceed to Cape Tscheluskin, and try to penetrate as far as possible in a north-easterly direction.

MR. G. J. HINDE, of Toronto, Canada, writes us that a shock of earthquake, unusually severe for that part of the world, occurred along the valleys of the St. Lawrence and Ottawa Rivers, Lakes Champlain and St. George, and through New Hampshire, Vermont, and Western Massachusetts, at or near 2 A.M. of Sunday, the 4th instant. The limits along which it has been noticed are Pembroke on the Upper Ottawa to the north-west, Montreal on the east, Boston and Providence to the south-east, and Toronto to the west. The shock appears to have been most severe on the line of the Ottawa valley between Pembroke and Montreal, and between Ottawa city and Cape Vincent on the St. Lawrence, following in a general direction the outcrops of the Laurentian range. It was but very slightly felt at Toronto, but at Montreal the shocks are stated to have lasted twenty seconds, and to have shaken movable articles about the rooms.

THE following grants in aid of researches have been made this year by the Committee of Council on the report of the Scientific Grants' Committee of the British Medical Association:—Mr. Gaskell, in aid of a research on the reflex action of the vascular system and muscles and reflex vasomotor action generally, 30*l.*; Mr. Langley, in aid of a research on the changes produced in the salivary glands by nerve influence, 25*l.*; Dr. Rutherford, F.R.S., for a continued research on the action of Cholagogues, 50*l.*; Drs. Braidwood and Vacher, for engravings for illustrating the third report on the life history of contagium, 40*l.*; Mr. Pye in aid of a continued research for the investigation of the relation that the retinal circulation bears to that of the brain, 8*l.* 15*s.*; Mr. Bruce Clarke, in aid of a continued research on syncope and shock, 10*l.*; Mr. A. S. Lee, Heidelberg, in aid of a research on the quantitative determination of digestive products obtained by the action of pancreatic ferment upon the various albumens, 25*l.*; Dr. McKendrick, Glasgow, in aid of a continued research into the antagonism of drugs, 30*l.*; Dr. McKendrick, Glasgow, in aid of an investigation into the dialysis of

blood (renewed), 10*l.*; Dr. John Barlow, Muirhead Demonstrator of Physiology, Glasgow, in aid of an experimental investigation into the changes produced in the blood-vessels by alcohol, 10*l.*; Dr. Joseph Coats, Dr. McKendrick, and Mr. Ramsay, the committee upon the investigation of anaesthetics, 50*l.*; Dr. McKenzie, a research on pyæmia, 25*l.*; Mr. Callender, F.R.S., Dr. J. Burdon Sanderson, F.R.S., Dr. T. Lauder Brunton, F.R.S., and Mr. Ernest Hart, the committee appointed for the investigation of the pathology and treatment of hydrophobia, 100*l.* Total, 413*l.* 15*s.*

TELEGRAPH warnings are to be employed all over Paris for giving alarms of fires to all the fire-engine stations. The alarm is given by breaking a small pane of glass facing the streets, being a variation of the system employed on railways for signalling the engine-driver or guard.

IN the November session of the Berlin Geographical Society, Baron v. Richthofen was re-elected president. The evening was chiefly occupied by an address from Dr. Nachtigal, on the results of Stanley's lately accomplished expedition, which he regarded as the most prominent event among later African explorations. Prof. Orth gave a short description of a new method of cartography.

LIEUT. DE SEMELLÉ has intimated to the Paris Geographical Society that he intends to cross Africa from west to east, ascending the Niger and Binué, making for Lakes Albert and Victoria, and reaching the east coast at Mombasa or Malinda. He states that he has already obtained sufficient resources.

THE chemists of Berlin have been occupied lately in analysing the wares of the wine merchants, and no little excitement has been caused by the discovery that the entire stock of one of the largest houses dealing in wines for medicinal purposes, consisted entirely of artificially prepared mixtures of spirit and sugar solutions, flavoured with various herbs.

AT Leipzig a "General German Anti-Adulteration Society" has been formed, which has for its main object the prevention of the adulteration of food. A periodical is to appear, or has already appeared, as the organ of this society. At some fifty other German towns branch societies are being established. All political or religious matters are excluded from the programme of the society, while one of its statutes prescribes the special prosecution of the makers and sellers of so-called secret remedies and medicines.

IN evidence of the interest now being taken by Spain in scientific subjects we may draw attention to the *Boletín de la Institución Libre de Enseñanza* (Madrid, 1877), the first five numbers of which, from March 7 to June 17, now lie before us. We notice Geometría y morfología natural, Prof. De Linares; Investigación de las propiedades ópticas, Prof. Calderon; La religión de los Celtas españoles, Prof. Costa; Principios y Definiciones de la Geometría, Prof. Jiménez; Precipitación de los metales puros por los sulfuros naturales, Prof. Quiroga. There are accounts of papers read at meetings under the headings "Resúmenes de Enseñanzas," and "Conferencias." The *Boletín* is in shape not quite so large as NATURE, and each number contains four pages.

THE Minister of Instruction in the cabinet chosen by Marshal MacMahon last week is M. A. E. A. Faye, the well-known astronomer, who is spoken of as Leverrier's probable successor. M. Faye is at present in his sixty-third year, and is chiefly known through his discovery of the comet named after him, in 1843. Since that time he has devoted his attention principally to the consideration of the problems of physical astronomy, the solar constitution, &c. His most important works are "Leçons de Cosmographie," 1852; and a translation of Humboldt's "Cosmos." M. Faye is probably the best known in what is

ironically termed the *cabinet des inconnus*. French politics allure an unusually large number of scientific men. Naquet, the chemist, is now a leader of the radical wing of the Republican party, Dumas and Scheurer-Kestner are life members of the senate, and Wurtz was proposed as a candidate for the senate a few weeks since.

THE communication of the city of Moscow with the river Volga, leaving the railway out of account, was, up to the present, only possible in the spring of each year, on account of the shallowness of the Moskwa River. The boats were drawn by horses from Moscow to Kolomna on the river Oka, which falls into the Volga at Nishni-Novgorod, and this means of communication, on account of the great time it occupied, not to mention its cost, was a very imperfect one. A series of locks has recently been constructed on the Moskwa River, and tug steamers are now running between the capital and the Oka.

WE have already referred to the proposed introduction of the telephone into the German telegraphic service. Dr. Stephan, the enterprising Postmaster-General of the German empire, who has brought the German postal service to such efficiency, and fairly created the present international telegraphic system, appears to have definitely settled the question of the practicability of the general introduction of the new method. For the past few weeks the telephone has been in constant use between the General Post Office and the General Telegraph Office in Berlin, and has superseded the telegraphic communication between Berlin and some of the neighbouring villages. The results have been so satisfactory that a few days since a consultation of leading telegraphic officials was held to make arrangements for the establishment of a large number of telephonic stations. Since the equipment of these stations is so inexpensive, and the long and costly preliminary training of a telegrapher is avoided, it can easily be understood with what readiness the new invention is put into practical use. Interesting in this connection is the recent adoption of the telephone by Prince Bismarck. He has caused, as we stated last week, the establishment of a telephonic means of communication between the Chancellor's office in Berlin and his country residence at Varzin, in Pomerania, 230 miles distant; and finds that he is perfectly able to give instructions and receive reports without leaving his favourite castle. No subterranean wires, but the ordinary telegraphic wires on poles, are used for this purpose.

A SERIES of researches on the compressibility of liquids has recently been described by M. Amagat in the *Annales de Chimie et de Physique*. Among other results, the compressibility is found to be far from depending on the volatility of liquids, as might be supposed. The presence of sulphur, chlorine, bromine, and probably also iodine, tends to diminish the compressibility (a fact sufficiently explained by the corresponding increase of density). With regard to alcohols, the compressibility diminishes from the first member of the series, methylic alcohol, at least at 100°. At 14° common and methylic alcohol have nearly the same compressibility; and at zero the common alcohol is perhaps more compressible than methylic alcohol. Of the ethers, ethyl-acetic ether is more compressible at 14° and at 100° than methyl-acetic ether (an inverse order to that of the densities, which decrease as you rise in the series). With regard to hydrocarbons, the compressibility decreases regularly both at ordinary temperature and at 100° as you descend in the series.

A MICROSCOPICAL study has recently been made by M. Prilieieux, of a disease of fruits, and especially of pears, which consists in the appearance of spots, then of crevices, issuing in complete disorganisation. From the facts described, it appears that the cause of this evil is a fungus, the spores of which are developed on the skin of the fruit with the appearance of a thin filament. At a certain time this filament penetrates the eider-

mis and produces a mycelium, which develops in the very mass of the fleshy tissue. Later there appear, in addition, fructiferous filaments, which bear about twenty-five spores each. The cells of the fruit, on passage of the parasite, are destroyed, and it is thus that the crevices are formed.

THE diffusion which takes place between two gases separated from each other by an absorbent film (*e.g.*, a soap film) was studied a short time ago by Prof. Exner, of the Vienna Academy. He has recently extended his inquiry to the case of vapours from easily volatile liquids, using the same apparatus as for permanent gases. The experiments were made with sulphide of carbon, chloroform, sulphuric ether, benzine, alcohol, and oil of turpentine, and they show that the diffusion from such vapours follows the same laws as those of gases, *i.e.*, that it depends both on the coefficient of absorption of the film and on the density of the gas being directly proportional to the former, and inversely proportional to the square root of the latter. Thus it appears that the greater or less distance of a gas from its liquefaction point has at least no influence on this kind of diffusion.

IT is reported that Herr Josef Albert, the eminent Munich photographer, has made the highly important invention of photographing the natural colours of objects by means of a combination of the ordinary photographic process with a photographic printing press constructed by the same gentleman some time ago. The images are stated to be so perfect that not the least improvement with the brush is required, as the finest shades of colours are faithfully reproduced. The secret of the invention is said to be based on the separation of white light into yellow, blue, and red rays, and in the artificial application of the same colours in the printing press. The first negative is taken upon a plate which is chemically prepared in such a manner that it only receives the yellow tints or shades of the object; this is then passed through the printing press, the roller of which is impregnated with a yellow colouring matter. On the print only the yellow tints reappear more or less distinctly; the object is then again photographed, and this time a negative is prepared which only receives the blue shades and tints; a second printing press has its roller impregnated with some blue colour, and the plate of course gives a print with only the blue tints reproduced. In the same manner a third print is obtained which only shows the red shades and tints. The final manipulation now consists in printing the three images upon the same plate, when the three colours intermingle and the natural colours and shades of the objects are obtained. We need hardly point out the enormous importance of this invention.

A PAMPHLET just published by the Director of the Paris National Library contains some interesting statistical data respecting one of the finest libraries in the world. It has been found that the library contains 86,774 volumes on catholic theology, 44,692 volumes on the science of languages, 289,402 volumes on law, 68,483 volumes on medicine, 441,836 volumes on French history, and 155,672 volumes of poetry. The works on natural science are not yet catalogued. During 1876 the library received no less than 45,300 French additions and purchased 4,565 foreign books.

THE additions to the Zoological Society's Gardens during the past week include two Black-eared Marmosets (*Hapale penicillata*) from South America, presented by Miss Quain; a Black-backed Jackal (*Canis mesomelas*) from South Africa, presented by Capt. Fulton, s.s. *Taymouth Castle*; a Common Boa (*Boa constrictor*) from South America, presented by Miss Alice Leith; a Brown Tree Kangaroo (*Dendrolagus inustus*) from New Guinea, a Slow Loris (*Nycticebus tardigradus*) from Malacca, a River Jack Viper (*Vipera rhinoceros*) from West Africa, purchased; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, deposited.

## THE LIBERTY OF SCIENCE IN THE MODERN STATE<sup>1</sup>

### II.

IT is easy to say: "A cell consists of small particles, and these we call plastidules; plastidules, however, are composed of carbon, hydrogen, oxygen, and nitrogen, and are endowed with a special soul; this soul is the product or the sum of the forces which the chemical atoms possess." Indeed this is possible; I cannot judge of it exactly. This is one of those points which are yet unapproachable for me; I feel there like a navigator who gets upon a shallow, the extent of which he cannot guess. But yet I must say that before the properties of carbon, hydrogen, oxygen, and nitrogen are defined to me in such a manner that I can understand how, through their combination a soul results, I cannot admit that we are justified in introducing the plastidule soul into the educational programme or to ask generally of every educated man that he should recognise it as a scientific truth to such a degree as to operate with it logically, and to base his conception of the universe upon it. This we may really not ask. On the contrary, I think that before we designate such theses as the expression of science, before we say this is modern science, we ought first of all to complete a whole series of lengthy investigations. *We must therefore say to the schoolmasters, do not teach this.* This, gentlemen, is the resignation which in my opinion, those ought to exercise who deem such a solution in itself to be the probable end of scientific investigation. We can certainly not differ on that point for a moment, that if this doctrine of the soul were really true it could only be confirmed by a long series of scientific investigations.

There is a series of events in the field of the natural sciences, by which we can show, for how long certain problems are in suspense, before it is possible to find their true solution. If this solution is found at last, and found in a direction of which there was a presentiment perhaps centuries ago, it does not follow that during those times which were occupied only by speculation or presentiment the problem might have been taught as a scientific fact.

Prof. Klebs spoke of *contagium animatum* the other day, *i.e.* the idea that in diseases the transmission takes place by means of living organisms, and that these organisms are the causes of contagious diseases. The doctrine of *contagium animatum* loses itself in the obscurity of the middle ages. We have had this name handed down to us by our forefathers, and it is very prominent in the sixteenth century. Certain works of that period exist, which put down *contagium animatum* as a scientific dogma with the same confidence, with the same kind of justification, as nowadays the plastidule soul is set up. Nevertheless the living causes of diseases could not be found for a long time. The sixteenth century could not find them, nor could the seventeenth and the eighteenth. In the nineteenth century we have begun to find some *contagia animata* bit by bit. Zoology and botany have both contributed to them: we have found animals and plants which represent contagia, and a special part of the knowledge of contagia has been absorbed into zoology and botany, quite in the sense of the theories of the sixteenth century. But you will already have seen from the address of Prof. Klebs that the end of proofs has not yet ended. However much we may be disposed to admit the general validity of the old doctrine, now that a series of new living contagia have been found, now that we know cattle disease and diphtheria to be diseases which are caused by special organisms, still we may not yet say that now *all* contagia or even all infectious diseases are caused by living organisms. After it has appeared that a doctrine, which was formulated already in the sixteenth century, and which has since obstinately emerged again and again in the ideas of men, has at last, since the second decade of the present century, obtained more and more positive proofs for its correctness, we might really think that now it was our duty to infer, in the sense of an inductive extension of our knowledge, that all contagia and miasmata are living organisms. Indeed, gentlemen, I will admit that this conception is an extremely probable one. Even those investigators, who have not yet gone so far as to regard the contagia and miasmata as living beings have yet always said that they resemble living beings very closely, that they have properties which we otherwise know in living beings only, that they propagate their kind, that they increase

<sup>1</sup> Address delivered at the Munich meeting of the German Association, by Prof. Rudolf Virchow, of Berlin. Continued from p. 74.

and are regenerated under special circumstances, that, indeed, they appear like real organic bodies,—these men, nevertheless, have waited, and rightly, until the proof of their being living organisms was furnished. And thus caution commands reserve even now.

We must not forget that the history of science presents a number of facts which teach us that very similar phenomena may happen in a very different manner. When fermentation was reduced to the presence of certain fungi, when it was known that its beginning was closely connected with the development of certain species of fungi, then it was really very obvious to imagine that all processes related to fermentation happen in the same way; I mean all those processes which are comprised under the name of "catalytic," and which occur so frequently in the human and animal body as well as in plants. There were, indeed, some scientific men who imagined that digestion, which is one of the processes which closely resemble the fermentative ones, was brought about by certain fungi which occur frequently (in the special case of cattle the question has been practically discussed), and which were supposed to cause digestion in the stomach in the same way as the fermentation fungi cause fermentation elsewhere. We now know that the digestive juices have absolutely nothing to do with fungi. Much as they may possess catalytic properties, we are yet certain that their active substances are chemical bodies which we can extract from them, which we can isolate from their other component parts, and which we can cause to act in the isolated state free from any admixture of living organisms. If the human saliva has the property of being able to change starch and dextrine into sugar in the shortest time, and if every time we eat bread this new formation of "sweet" bread takes place in our mouth, then no fungus takes part in this nor any fermentation organism, but there are chemical substances which, much in the same way as it happens in the interior of the fungus, bring about chemical change in matter. We see, therefore, that two processes which are extremely similar, the one in the interior of the fermentation fungus and the other in the process of human digestion, are brought about in different ways; the same process in the one instance is connected with a certain vegetable organism, while in the other it takes place without any such organism and simply through a liquid.

I should consider it a great misfortune if we were not to continue in the same way as I have done now, to examine in each single case whether the *supposition* which we make, the *idea* which we have formed and which may be highly probable, is really true, whether it is justified *by facts*. With regard to this I would remind you that there are cases also amongst the infectious diseases where most undoubtedly a similar contrast exists. My friend, Prof. Klebs, will no doubt pardon me if I, even now, in spite of the recent progress which the doctrine of infecting fungi has made, still remain in my reserve, and that I only admit that fungus which has been proved by demonstration, while I deny all the other fungi as long as I do not hear of facts which attest them. Amongst infectious diseases there is a certain group which are caused by organic poisons—I will only mention one of them, which, according to my opinion, is very instructive—I mean the poisoning by a snake-bite, a very celebrated and most remarkable form. If this kind of poisoning is compared with those kinds of poisoning which are generally called infectious diseases (infection does not signify much else than poisoning), then we must admit that in the courses both cases generally take the greatest analogies exist. With regard to the course of the illness nothing would oppose the supposition that the total sum of phenomena which occur in a human body after a snake-bite, were caused by fungi which entered the body and which produced certain changes in different organs. Indeed we know certain processes, septic ones, for instance, where phenomena of a completely similar nature occur, and it cannot be denied that certain forms of poisoning by snake-bite resemble certain forms of septic infection as much as one egg resembles another. And yet we have not the least cause to suspect an importation of fungi into the body in the case of snake bite, while in the case of septic processes we, on the contrary, acknowledge and recognise this importation.

The history of our natural science has numerous examples, which ought always to cause us more and more to confine the validity of our doctrines in the most stringent manner to that domain only in which we can actually prove them, and that we do not by way of induction, proceed so far as to extend doctrines immeasurably which have only been proved for one or several cases. Nowhere the necessity of such a restriction has become

more apparent than on the field of the theory of evolution. The question of the first origin of organic beings, this question which also forms the basis of progressive Darwinism, is an extremely old one. It is not known at all who first tried to find the different solutions for it. But if we remember the old popular doctrine, according to which all possible beings alive, animals and plants, could originate from a clod of clay—from a little clod under circumstances—then we ought to remember at the same time that the celebrated doctrine of *generatio equivoca*, of epigenesis, is closely connected with it, and that it has been a common idea for thousands of years. Now with Darwinism the doctrine of spontaneous generation has been taken up again, and I cannot deny that there is something very seductive in the idea of closing the theory of descent in this way, and, after the whole series of living forms has been constructed, from the lowest protozoa upwards to the highest human organism, to connect this long series with the inorganic world as well. This corresponds with that direction to generalise, which is so entirely human, that it has found a place in the speculation of mankind at all times, backwards to the most obscure periods. We have the undeniable desire not to separate the organic world from the universe, as a something which is divided from it, but rather to insure its connection with the universe. In this sense it is pacifying if one can say, the atom-group carbon and company—this is perhaps speaking too collectively, but yet it is correct, since carbon is to be the essential element—therefore, this association, carbon and company, has at some special time separated itself from the ordinary carbon and founded the first plastidule under special circumstances, and continues to found it in the present. But in the face of this we must mention that all real scientific knowledge of the phenomena of life has proceeded in an opposite direction. We date the beginning of our real knowledge of the development of higher organisms from the day when Harvey pronounced the celebrated phrase, "Omne vivum ex ovo," every living being comes from an egg. This phrase as we now know, is incorrect in its generality. To-day we can no longer recognise it as a fully justified one; we know that, on the contrary, a whole number of generations and propagations exist without ova. From Harvey down to our celebrated friend Prof. von Siebold, who obtained the general recognition of parthenogenesis, there lies a whole series of increasing restrictions, all of which prove that the phrase, "Omne vivum ex ovo," was incorrect speaking in a general sense. Nevertheless, it would be the highest ingratitude if we were not to acknowledge that in the opposition, which Harvey assumed against the old *generatio equivoca*, the greatest progress was made which has been made by science in this domain. Later on a great number of new forms were known, in which the propagation of the different kinds of living beings is going on, in which new individuals originate—direct separation, gemmation, metagenesis. All these forms, parthenogenesis included, are data which have caused us to give up every single (*einheitliche*) system for the generation of organic individuals. In place of a single scheme we now have a variety of data; we have no uniform system left by which we could explain once for all how a new animal begins.

*Generatio equivoca*, which has been disputed and refuted as many times, nevertheless faces us again and again. It is true that not a single *positive fact* is known which proves that *generatio equivoca* has ever occurred, that spontaneous generation has ever taken place in such a way that inorganic masses, let us say the association carbon and company, have ever spontaneously developed into an organic substance. Nevertheless, I admit that if we indeed want to form an idea how the first organic being could have originated by itself, nothing remains but to go back to spontaneous generation. This is clear. If I do not want to suppose a creation-theory, if I do not want to believe that a special creator existed, who took the clod of clay and blew his living breath into it, if I want to form some conception in my own way, then I must form it in the sense of *generatio equivoca*. *Tertium non datur*. Nothing else remains if once we say "I do not admit creation, but I do want an explanation." If this is the first thesis, then we must proceed to the second and say "Ergo, I admit *generatio equivoca*." But we have no actual proof for it. Nobody has ever seen *generatio equivoca* occurring in reality, and everyone who maintained that he had seen it, has been refuted, not by theologians indeed, but by naturalists. I mention this, gentlemen, in order to let our impartiality appear in the right light, and this is very necessary at times. We always have our weapons in ourselves and about us, to fight against that which is not justified.

I therefore say that I must admit the theoretical justification

of such a formula. Whoever will have a formula, whoever says "I absolutely want a formula, I wish to be perfectly at one with myself, I must have a coherent conception of the universe," must either admit *generatio equivoca* or creation; there is no other alternative. If we want to be outspoken we may indeed own that naturalists may have a slight predilection for *generatio equivoca*. It would be very beautiful if it could be proved.

But we must admit that it is not yet proved. Proofs are still wanting. If any kind of proof were to be successfully given we would acquiesce. But even then it would have to be determined first, to what extent we could admit *generatio equivoca*. We should quietly have to continue our investigations, because nobody will think that spontaneous generation is valid for the totality of organic beings. Possibly it would only apply to a single series of beings. But I believe we have time to wait for the proof. Whoever remembers in what a regrettable manner, quite recently, all attempts to find a certain basis for *generatio equivoca* in the lowest forms of the transition from the inorganic to the organic world, have failed, should consider it doubly dangerous to demand that this ill-reputed doctrine should be adopted as a basis for all human conceptions of life. I may, doubtless, suppose that the story of the *Bathybius* has become known to nearly all educated persons. With this *Bathybius* the hope has again vanished that *generatio equivoca* can be proved.

I think, therefore, that with regard to this first point, the point of the connection between the organic and the inorganic, we must simply own that in reality we know nothing about it. We may not set down our supposition as a certainty, our problem as a dogma; that cannot be permitted. Just as in the progress of the doctrines of evolution it has been far more certain, more fertile, and more in accordance with the progress of accredited natural science, to analyse the original single doctrine part by part, we shall also have first to keep apart the organic and inorganic things in the old well-known analysing way, and not to throw them together prematurely.

Nothing, gentlemen, has been more dangerous to natural science, nothing has done more harm to its progress and to its position in the opinion of nations than premature syntheses. While laying stress upon this, I would point out specially how our Father Oken was damaged in the opinion not only of his contemporaries, but also in that of the following generation, because he was one of those who admitted syntheses into their conceptions to a far greater extent than a stricter method would have allowed. Do not let us lose the example of the natural philosophers; do not let us forget that every time that a doctrine which has assumed the air of a certain, well-founded, and reliable one, of one which claims general validity, turns out to be faulty in its outlines, or is found to be an arbitrary and despotic one in essential and great points, then a great number of men lose their faith in science entirely. Then the reproaches begin—"You are not sure even yourselves; your doctrine, which is called truth to-day, is a falsehood to-morrow; how can you demand that your doctrine shall become the object of instruction and of the general consciousness?" From such experiences I take the warning that if we wish to continue to claim the attention of all we must resist the temptation of pushing our supposition, our merely theoretical and speculative structures into prominence to such a degree that from them we would construct the conception of the whole remaining universe.

(To be continued.)

#### THE METEOR

A METEOR of unusual brilliancy was seen on the evening of Friday, the 23rd inst., from various parts of the kingdom. Mr. F. A. Buxton writing to us from Hertford states that he saw it two miles north of that town at 8.26 P.M. He says:—"I was attracted by its glare notwithstanding the moonlight, and saw it moving vertically downwards. I could not accurately observe its path, but it passed, nearly or exactly, over a small star, just visible in the moonlight, which I think is  $\pi$  Herculis, and disappeared suddenly before it reached the horizon, in about N.P.D. 60 and R.A. 16°40'. By comparing notes with another observer (half a mile north of Hertford) it appears to have been visible much nearer the zenith than I had seen it; probably I saw the last 15° of its path. From the apparent slowness of its motion and complete absence of sound I gather that it was far off. My guess at the moment was fifty miles. In consequence of its brightness its apparent diameter was probably illusory. It attained two *maxima* of splendour, one about over the star

named, the other at its disappearance. Scarcely any 'trail' was left; what there was almost immediately vanished."

Mr. T. Mellard Reade writes that he saw it from Blundellsands, Liverpool, at 8.20 P.M. Looking up he saw a splendid broad streak of blue light terminating in a ball of red fire rushing across the sky in a north-westerly direction. The first flash seemed directly overhead; if so, Mr. Reade states, the meteor must have travelled through at least 45°. Shortly afterwards the moon being intensely bright and a shower coming on from the west, across the sea a most splendid "moon" rainbow made its appearance, finishing as a perfect arch of vivid colours with a second and a perfect bow above it.

Mr. W. B. Ferguson writes from Edinburgh that while walking down Princes Street about 8.25 P.M. he saw a most brilliant meteor which appeared to fall almost vertically and burst with great brilliancy apparently just behind the castle. Its direction from where he observed it was 10° west of south.

Mr. C. H. Dance, writing from Manor House, Ardwick, Manchester, gives the time as 8h. 25m. Greenwich mean time. The meteor, he states, appeared to come from the constellation Cassiopeia, and after travelling in a direction a little to the west of north, finally burst behind a cloud about thirty degrees above the horizon. The apparent size of the meteor was considerably greater than that of Mars during the late opposition, and the light which it emitted was intensely bright and of a bluish-green colour, leaving a decidedly red impression on the retina. The period of visibility would be about five seconds, and the sparks in the train were also visible for some seconds.

Mr. Plant, the Curator of the Salford Museum, observed the meteor at the same time, visible to the north of Manchester.

Dr. S. Drew, of Sheffield, saw it at about 8.30 P.M. He gives the apparent diameter as two minutes; path, from the square of Pegasus to near Altair; motion, slow; shape, at first globular, afterwards elongated, with tail. It then appeared to break up. Colour, at first blue-green, afterwards ruddy; light, brilliant. He heard no sound accompanying the meteor, and from the absence of sound and slow apparent motion, he infers the real distance and size of the bolide to have been great. Dr. Drew was, at the time of observation, a little to the west of the town of Rotherham.

Several correspondents write to the *Times* describing what they saw of this remarkable meteor, for it is evidently the same body which has been seen by the various observers. The Liverpool correspondent of the *Times* saw it about 8.30. "A large ball of fire shot from the sky, exploding and throwing off innumerable variegated sparks as it descended in a northerly direction. The track of sparks gave the meteor the appearance of a brilliant comet with a long tail. Some spectators state that they heard the hissing noise made in its course, and others allege that it descended into the water near the bar of the Mersey with a great noise, sending up a column of steam and spray."

Mr. Donald Mackay saw it from Victoria Street, London, shortly before 8.30 P.M. "It travelled with great rapidity for about 20° from the zenith to the horizon, bursting in a white ball as large as twelve of the planet Mars in one, lighting up all the houses surrounding Victoria Street, the point of observation, and leaving a large tail behind the shape of a spear-head, with all the colours of the rainbow in it."

The Rev. J. Hoskyns-Abrahall writes from Combe Vicarage, near Woodstock, that about 8.20 the northern sky was suddenly lighted up with a glow that outshone that spread over the south-eastern sky by a moon nearly full. "Looking northwards I saw a globular meteor of a pale orange colour descending perpendicularly. Its apparent size was scarcely less than that of the moon. Just above the slope on which I was, and seemingly not half a mile off, it burst into huge fragments, which flared forth with a fierce, lightning-like, reddish glare, and scattered sparks of surpassing splendour."

Mr. D. Aldred writes from Milford, Derby, to the same effect. He saw the meteor about six miles north of Derby, about 8.25. "It was almost due north, and travelling from the zenith to the horizon, the point of dispersion being about 45° above the north point of the horizon. In shape it was conical, the greatest breadth about one and a half times the diameter of the moon. It left a trail of considerable length, and the colours detached were of most remarkable brilliancy."

"R. M. C." writes from Cathedine, Brecknockshire, giving the report of two reliable witnesses who were walking in an easterly direction at 8.25 P.M. Looking back, the moon being at the time obscured by a cloud, they saw a ball of the most intense white light, "about the size of a cannon-ball," travers-

ing a space between two clouds, leaving behind it a fiery track of red.

A Worcester correspondent gives the time as 8.20. He describes the colour as brilliant blue and orange, and behind was a streaming trail of brilliant sparks, which remained visible for a few seconds after the brighter light had disappeared.

#### UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At a Congregation on November 22, the University seal was ordered to be affixed to a letter of thanks to his Grace the Chancellor of the University for his munificent gift of a complete apparatus of scientific instruments for the Cavendish Laboratory.

A meeting of the members of the University to consider the propriety of securing a personal memorial of Dr. Darwin, was held on Monday in the combination room of Christ's College, the Rev. Dr. Cartmell, Master of the College, presiding. It was proposed by Prof. Humphry and seconded by Prof. Fawcett, "That it is desirable that the University should possess a personal memorial of Mr. Charles Darwin, LL.D." Proposed by Prof. Newton and seconded by Mr. Piele, of Christ's, "That the members of the University now present form themselves into a committee, with power to add to their number, for the purpose of collecting subscriptions from members of the University to carry out the foregoing resolution." Proposed by Prof. Liveing, seconded by Mr. J. W. Clark, "That Mr. A. G. Dew-Smith, of Trinity College, be treasurer and secretary to the committee, and be authorised to receive subscriptions." It was understood that the memorial should assume the form of a portrait, and about 75*l.* was subscribed in the room.

EDINBURGH.—The subscriptions to the Edinburgh University Extension Fund now amount to 82,000*l.*, and Government has now promised to add 80,000*l.* to the amount on condition that 25,000*l.* is raised by public subscription, of which the sum of 10,000*l.* must be subscribed by December 31st next. The University Professors at Edinburgh have already contributed among themselves 5,360*l.* towards the additional 25,000*l.* required.

ST. ANDREWS.—Lord Selborne has been elected Lord Rector of this University. The students had much difficulty in getting any eminent man to allow himself to be nominated, and it was only on the day previous to the election that it was resolved to pit Lord Selborne against the Right Hon. Gathorne Hardy.

Prof. Alleyne Nicholson has been appointed Swinburne Lecturer on Geology by the Trustees of the British Museum.

LEIPZIG.—Prof. Leuckhart, the newly-elected Rector of the University, was installed into the duties of the office on October 31, and delivered on the occasion an able address "On the Development of Zoology up to the Present Time, and its Importance." The students already number nearly 3,200, an attendance, as usual, far above that of any other German university.

AMSTERDAM.—The new University of Amsterdam has lately made a most flattering offer to Prof. Gegenbaur, of Heidelberg, which has, however, been declined.

BERGEN.—It is intended to establish a new university in the Norwegian town of Bergen. Eighty thousand crowns have already been subscribed towards this object.

#### SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, November 8.—Lord Rayleigh, F.R.S., president, in the chair.—The following were elected to form the Council during the session:—President: Lord Rayleigh, F.R.S. Vice-Presidents: Prof. J. Clerk Maxwell, F.R.S., Mr. C. W. Merrifield, F.R.S., Prof. H. J. S. Smith, F.R.S. Treasurer, Mr. S. Roberts. Hon. Secretaries: Messrs. M. Jenkins and R. Tucker. Other members, Prof. Cayley, F.R.S., Mr. T. Cotterill, Mr. J. W. L. Glaisher, F.R.S., Mr. H. Hart, Dr. Henrici, F.R.S., Dr. Hirst, F.R.S., Mr. Kempe, Dr. Spottiswoode, F.R.S., Mr. J. J. Walker.—Prof. Cayley made two communications, on the function  $\phi(x) = \frac{ax+b}{cx+d}$  (a singularly neat expression was got for  $\phi^n(x)$ , the late Mr.

Babbage had considered the matter in 1813), and on the theta functions.—Mr. Tucker read a portion of a paper by Mr. Hugh MacColl (communicated by Prof. Crofton, F.R.S.) entitled the calculus of equivalent statements. A short account of this analytical method has been given in the July and November numbers (1877) of the *Educational Times*, under the name of Symbolical Language. The chief use at present made of it is to determine the new limits of integration when we change the order of integration or the variables in a multiple integral, and also to determine the limits of integration in questions relating to probability. This object, the writer asserts, it will accomplish with perfect certainty, and by a process almost as simple and mechanical as the ordinary operations of elementary algebra.—The president read a paper on progressive waves. It has often been remarked that when a group of waves advance into still water the velocity of the group is less than that of the individual waves of which it is composed; the waves appear to advance through the group, dying away as they approach its anterior limit. This phenomenon seems to have been first explained by Prof. Stokes, who regarded the group as formed by the superposition of two infinite trains of waves of equal amplitudes and of nearly equal wave-lengths advancing in the same direction. The writer's attention was called to the subject about two years since by Mr. Froude, and the same explanation then occurred to him independently. In his work on "The Theory of Sound" (§ 191), he has considered the question more generally. In a paper read at the Plymouth meeting of the British Association (afterwards printed in NATURE), Prof. Osborne Reynolds gave a dynamical explanation of the fact that a group of deep-water waves advances with only half the rapidity of the individual waves. Another phenomenon (also mentioned to the author by Mr. Froude) was also discussed as admitting of a similar explanation to that given in the present paper. A steam launch moving quickly through the water is accompanied by a peculiar system of diverging waves, of which the most striking feature is the obliquity of the line containing the greatest elevation of successive waves to the wave-fronts. This wave-pattern may be explained by the superposition of two (or more) infinite trains of waves, of slightly differing wave-lengths, whose direction and velocity of propagation are so related in each case that there is no change of position relatively to the boat. The mode of composition will be best understood by drawing on paper two sets of parallel and equidistant lines, subject to the above conditions, to represent the crests of the component trains. In the case of two trains of slightly different wave-lengths, it may be proved that the tangent of the angle between the line of maxima and the wave-fronts is half the tangent of the angle between the wave-fronts and the boat's course.—Prof. Clifford, F.R.S., communicated three notes. (1) On the triple generation of three-bar curves. *If one of the three-bar systems is a crossed rhomboid, the other two are kites.* This follows from the known fact that the path of the moving point in both these cases is the inverse of a conic. But it is also intuitively obvious as soon as the figure is drawn, and thus supplies an elementary proof that the path is the inverse of a conic in the case of a kite, which is not otherwise easy to get. (2) On the mass-centre of an octahedron. The construction was suggested by Dr. Sylvester's construction for the mass centre of a tetrahedral frustum. (3) On vortex-motion. The problem solved by Stokes as a general question of analysis, and subsequently by Helmholtz for the special case of fluid motion may be stated as follows: given the expansion and the rotation at every point of a moving substance, it is required to find the velocity at every point. The solution was exhibited in a very simple form.

Zoological Society, November 6.—Mr. A. Grote, vice-president, in the chair.—A letter was read from Mr. R. Trimen, containing remarks on the African species of *Sarcidionis*.—A letter was read from Mr. A. O. Hume, containing some remarks on Mr. Howard Saunders' recent paper on the Sterninae.—The secretary exhibited, on the part of Mr. Geo. Dawson Rowley, an egg of *Pausis galeata*, laid by a black female.—Prof. W. H. Flower, F.R.S., read a paper entitled "A Further Contribution to the Knowledge of the existing Ziphioid Whales of the Genus *Mesoplodon*, containing a Description of a Skeleton and several Skulls of Cetaceans of that Genus from the Seas of New Zealand."—A communication was read from Lieut.-Col. R. H. Beddoe, containing the descriptions of three new species of reptiles from the Madras Presidency. These were proposed to be called *Oligodon travancoricum*, *Gymnodactylus jayporensis*, and *Bufo travancoricus*.—A communication was read from the Marquis of Tweeddale, F.R.S., containing an account of a collection of

birds made by Mr. A. H. Everett in the Island of Luzon, Philippines. Three new species were named *Megalurus ruficeps*, *Dicaeum xanthopygum*, and *Oxyterea everetti*.—Mr. D. G. Elliott read some remarks on *Felis tigrina*, Erx., and its synonymy, showing that *F. mitis*, F. Cuv., and *F. macrura*, Pr. Max., are identical with that species.—Prof. Garrod, F.R.S., read a paper on some points in the visceral anatomy of the rhinoceros in certain storks.—Mr. Edgar A. Smith read a paper in which he described some shells from Lake Nyassa, and a few marine species from the mouth of the Macusi River, near Quillimane, on the East Coast of Africa.—A communication from Dr. O. Finsch contained the description of a new species of petrel from the Feejee Islands, which it was proposed to name *Procellaria albicularis*.—A second communication from Dr. Finsch contained a report on the collections of birds made during the voyage of H.M.S. *Challenger* at Tongatabu, the Fiji Islands, Api, New Hebrides, and Tahiti.—Mr. Edward R. Alston read a supplementary note on rodents and marsupials from Duke of York Island and New Ireland. *Macropus lugens*, Alst., was shown to be a synonym of *Helmatomys brownii*, Ramsay, while Mr. Ramsay's *Mus. echinoides* and *M. musavora* were respectively identical with *Mus. brownii* and *Uromys rufescens* of Alston.—A communication from Mr. L. Taczanowski contained a supplementary list of birds collected in North-Western Peru by Messrs. Jelski and Stolzmann. Two species were new, and proposed to be called *Rallus cypereti* and *Penelope albipennis*.

## CAMBRIDGE

**Philosophical Society**, October 22.—A communication was read by Mr. Balfour, on the development of the vertebrate ovum. The points dealt with in this paper were (1) the nature of the stroma of the ovary, and (2) the relation of the permanent ova to the large cells of the germinal epithelium, named primitive ova by Waldeyer.

October 29.—Mr. Bonney read a paper on the rocks of the Lizard District (Cornwall). The author brought forward evidence to prove that the serpentine of this district was clearly intrusive among the hornblende schists.

November 5.—Prof. Clerk Maxwell communicated to the society an account of the unpublished papers of the Hon. Henry Cavendish, which contain his experiments in electricity.

## MANCHESTER

**Literary and Philosophical Society**, October 2.—Rev. William Gaskell, M.A., in the chair.—A case of flowering of *Chamopis fortunei* (Hook) at Alderley, by Arthur W. Waters, F.G.S. The fact of *Chamopis fortunei* (Hook) flowering so far north as near Manchester seemed to the author to be of sufficient interest to be worth mentioning to the Society.—Table of effect of movement of the surface of the globe on the shifting of the axis of the earth, by Arthur W. Waters, F.G.S.

## PARIS

**Academy of Sciences**, November 19.—M. Peligot in the chair:—The following papers were read:—Meridian observations of small planets at the Greenwich and Paris Observatories during the third quarter of 1877, communicated by M. Villarceau, —New remarks on the quantities of heat liberated by mixture of water with sulphuric acid, by M. Berthelot. He affirms that sulphuric acid always liberates the same quantities of heat whether it have been recently heated or kept a considerable time.—*Resume of a history of matter* (fifth article), by M. Chevreuil.—On the theory and the various manœuvres of the economising apparatus constructed at the dam of Aubois, by M. de Caligny.—On the use of refined neutral oils for lubrication of pistons in engines with surface condensers, by M. Allaire. Lime causes decomposition of neutral fatty matters and unites with their acids, the result being a greater deposit than if lime had not been used. Doubtless the deposit is oleate of lime instead of oleate of iron, and the boiler is preserved from attack; but the inconveniences in condensing engines are aggravated, for the condenser ceases to act as the tubes get covered. M. Allaire commends the use of refined neutral fatty matters which are undecomposable under the ordinary pressure of boilers.—Various observations on phylloxera, by M. Boiteau. The winter egg is deposited exclusively on the exterior of the stock.—Discovery of a small planet at

Ann Arbor, by Mr. Watson.—General map of the proper motions of stars, by M. Flammarion. One result of this comparison is contradictory of some common views as to the distance of stars relatively to their order of brightness; for the greatest proper motions do not belong to the most brilliant stars, but indifferently to all sizes. Again, the author cannot support Bessel's and Struve's view that double stars are carried through space more rapidly than simple stars.—On the equation with partial derivatives of the fourth order, expressing that the problem of geodesic lines, considered as a problem of mechanics, supposes an algebraic integral of the fourth degree, by M. Levy.—New applications of a mode of plane representation of classes of ruled surfaces, by M. Mannheim.—On the laws which rule the order (or class) of plane algebraic curves, of which each point (or each tangent) depends at once on a variable point and tangent in a given curve, by M. Fouret.—Extract from a letter (mathematical) to M. Hermite, by M. Fuchs.—On the decomposition into first factors of the numbers  $2^n \pm 1$ , by M. de Longchamps.—Reproduction of orthose, by M. Hautefeuille. Orthose can be obtained by raising to from 900 to 1,000 deg. a mixture of tungstic acid and a very alkaline silico-aluminate of potash containing one equivalent of alumina to six of silica. The tungstic acid forms tungstate of potash, and the silico-aluminate is thus brought to the composition of orthose.—On the composition and industrial use of gases from metallurgical furnaces, by M. Cailletet. These gases, if suddenly cooled, are found to contain an important quantity of combustible principles which can easily be lit again and burnt by passing, e.g., through a grate with burning fuel, and having their velocity diminished.—Formation of iodous acid by the action of ozone on iodine, by M. Ogier.—On the solubility of sugar in water, by M. Courtoune. A saturated solution of sugar at  $12^{\circ}5$  contains 66.5 gr. per cent. of sugar; one at  $45^{\circ}$  contains 71 gr. per cent.—On the products of oxidation of camphor, by M. Montgolfier.—Note on the accessory discs of the thin discs in striated muscles, by M. Renant. Muscular striation is formed of a succession of thick discs alone contractile, and of clear bands traversed each by a thin disc and two accessory discs similar to each other as regards form, and probably having similar functions.—An algesia obtained by the combined action of morphine and chloroform, by M. Guibert. A subcutaneous injection of chlorhydrate of morphine is made at least fifteen minutes before inhalation of chloroform.—On the causes of violet colour in oysters of the basin of Arcachon, by M. Descout. The colour is found to be due to the presence of a small algae of the family of Rhodospiraceae and Florideae. This becomes more abundant in time of drought, and probably acts by absorbing moisture.—On the migrations and metamorphoses of the taenias of shrew mice, by M. Villot.—On certain monstrosities of *Asterocanthion rubens*, by M. Giard.—On the embryogeny on the cestoids, by M. Moniez.—On the bismuth ores of Bolivia, Peru, and Chili, by M. Domeyko.

## CONTENTS

	PAGE
FLORA OF MAURITIUS AND SEYCHELLES. By W. R. McNAB . . . . .	77
OUR BOOK SHELF:—	
Von Hauer's "Die Geologie". . . . .	78
LETTERS TO THE EDITOR:—	
Fritz Müller on Flowers and Insects.—CHARLES DARWIN, F.R.S. . . . .	78
The Radiometer and its Lessons.—G. JOHNSTONE STONEY : Prof. G. CAREY FOSTER, F.R.S. . . . .	79
Mr. Crookes and Eva Fay.—Dr. WILLIAM B. CARPENTER, F.R.S. . . . .	81
Potential Energy.—Prof. H. W. LLOYD TANNER . . . . .	81
Smell and Hearing in Moths.—GEORGE J. ROMANES ; J. C. . . . .	82
Meteorological Phenomenon.—JOSEPH JOHN MURPHY . . . . .	82
OUR ASTRONOMICAL COLUMN:—	
Stellar Systems . . . . .	82
The Minor Planets . . . . .	83
The Cordoba Observatory . . . . .	83
CARL VON LITTRW . . . . .	83
BACTERIA. By J. BURDON-SANDERSON, M.D., LL.D., F.R.S. . . . .	83
DIFFUSION FIGURES IN LIQUIDS. By Prof. TITO MARTINI (With Illustrations). . . . .	87
TRACES OF EARLY MAN IN JAPAN. By EDWARD S. MORSE . . . . .	89
NOTES . . . . .	89
THE LIBERTY OF SCIENCE IN THE MODERN STATE, II. By Prof. RUDOLF VIRCHOW . . . . .	92
THE METEOR. . . . .	94
UNIVERSITY AND EDUCATIONAL INTELLIGENCE . . . . .	95
SOCIETIES AND ACADEMIES . . . . .	95

